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BRAITHWAITE'S RETROSPECT.

VOL. LVII. JANUARY—JUNE, 1868.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D. Lond.

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SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR; INCLUDIN SEVERAL PAPERS NOT INSERTED IN THE BODY OF THE WORK.

AFFECTIONS OF THE SYSTEM GENERALLY.

AGUE.—*Strychnia.*—Five interesting cases are related illustrative of the action of quinine in certain types of ague. In the first case, quinine having entirely failed, one-sixteenth of a grain of strychnia was given four times a day. This "immediately checked the paroxysms, and the patient was able to return to his estate after having been only five days under treatment, and wrote a fortnight afterwards to say that he still continued free from fever." (Dr. J. P. Nash, H.M. Madras Army, p. 5.)

RHEUMATISM.—If one-fiftieth to one-fortieth of a grain of sulphate of atropia is injected in the neighbourhood of joints affected with rheumatism the pain is immediately relieved. (Dr. J. Harley, p. 389.)

Alcoholic Rheumatism.—This is quite a distinct form of disease from that which arises from exposure to cold and wet; neither is it gout: yet it has not hitherto been properly distinguished from either of these two diseases. It appears usually about the middle age of life, after the person has daily imbibed fermented alcoholic liquors, even in moderate quantities. There is an expression of dulness or stupidity in the countenance, a weakness of the intellect, a stiffness and sluggishness of the limbs, producing a peculiar hobbling gait. These symptoms increase with age. This complaint does not affect the general health. It may be seen in publicans, gentlemen's servants, agricultural labourers, and farmers, and is very common in the cider counties. (Mr. J. Higginbottom, p. 16.)

FUNGOID ORIGIN OF FEVERS AND CHOLERA.—The opinion is gradually gaining ground that the malarious fevers of hot climates, dysentery, yellow fever, and cholera, are all caused by the action of different species of fungus or of infusoria. The time is probably not far distant when further investigations will prove that enteric and typhus fevers, as well as scarlet

fever, measles, and small-pox, have a like origin. In the late fatal epidemic of fever in the Mauritius, minute plants of fungus were found along the whole of the intestinal canal, and these corresponded with growths found in the waters of the Grand River. These spores were not found in the blood of patients, nor have they been found in the blood of fever and cholera patients in this country. (Mr. R. Hamilton, p. 1.)

Organic Germs.—It is not the gases which result from putrefaction which are sources of danger to life, but the organic germs which are so abundantly produced by that process. The presence of these may be detected with the microscope, and are found to consist of monads, vibrios, amœbæ, and other of the lowest forms of life. These may be seen in the vapour condensed by placing a bell-jar over putrid meat. M. Chalvet finds that when watery vapour near a suppurating surface is collected, it is found to be charged with irregular corpuscles resembling dried pus. Eiselt, of Prague, found small cells like pus cells in the air of a ward in which epidemic opthalmia was raging. (Dr. W. Proctor, M. Chalvet, M. Eiselt, p. 18.)

Pyæmia.—Dr. Richardson has separated the poison of pyæmia; it may, he says, be evaporated to the form of syrup or extract. It produces, when dried, a substance closely resembling the snake poison. It admits of being pulverised, and when it is introduced into the wound of a healthy animal produces precisely the same symptoms as those of the patient from which the poison was taken. (Dr. W. Proctor, Dr. Richardson, p. 21.)

The Typhoid State in Different Diseases.—In the typhoid state, in whatever disease it is present, it is probable that the pathology is always identical. For instance, in cases of contracted kidney, it is generally admitted that the cerebral symptoms—the typhoid state—often indistinguishable from those of typhus, are owing to a poisoned state of blood from retention of the products of the retrograde metamorphosis of the nitrogenous tissues, which it is the function of the kidney to eliminate from the body. When convulsions, a symptom characteristic of an extreme typhoid state, occur in scarlatina, the cause is retention of the renal excreta from inflammation of the kidney, and consequent blood-poisoning. When similar symptoms occur in typhus, there is always albumen in the urine. These considerations point to the appropriate treatment, viz., depuration of the blood, at the same time avoiding all measures which are likely to irritate the kidneys. (Dr. Murchison, British Medical Journal, Jan. 4, 1868, p. 6.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CONIUM.—*Therapeutic Action.*—Hemlock has no cerebral action at all. It is to the corpora striata, the smaller nervous centres, and the whole of the motor tract, just what opium is to the brain, it tranquillizes and renovates the whole nervous system. Its full action is asleep. The neglect of conium as useless is attributable to its administration in too small doses. It is pre-eminently a children's remedy, and is of service in laryngismus stridulus, and tetanic convulsions. (Dr. J. Harley, p. 385.)

HYPNOTIC.—*Henbane in Combination with Opium.*—Hyoscyamine in combination with opium produces the most powerful hypnotic action possible. Each increases the effect of the other. Quantities of morphia and hyoscyamine, which of themselves are insufficient to produce sleep, will, when combined, speedily induce that condition. (Dr. J. Harley, p. 391.)

PARALYSIS.—*Hypodermic Injection of Strychnia.*—Strychnia has long been acknowledged to be the best remedy we have in cases of paralysis, but the advantages to be derived from its use by hypodermic injection far exceed those from its use in the ordinary way. We may expect to find a beneficial result produced in a shorter time, and from much smaller doses, than when it is given by the mouth ; and benefit may be expected even in cases which have been treated by its stomachic administration without benefit. It is well to commence with small doses, as the one-ninetieth of a grain, and if this is borne well, increased to one thirty-sixth of a grain, which is borne well by most patients. Some bear well one twenty-fourth of a grain. It should not be given when active irritation is going on about the clot or presumed cause of the lesion. About two days should elapse between each injection. The cases related are very interesting. One noteworthy symptom produced was a feeling of heat in the body generally. (Mr. C. Hunter, p. 25.)

TETANUS.—*Nicotine.*—A case of tetanus is related in which the effect of nicotine was very remarkable. The most noticeable was a diminution in the frequency of the heart's action. In four hours and three-quarters the pulse fell from 120 to 68. The patient also at the same time sweat profusely, whereas previously the skin had been hot and dry. There was also much less muscular rigidity than had previously existed, and he was enabled to pass his urine naturally, which he had not done for thirty hours. The case ended favourably under this treatment. In administering the nicotine it was found that

one twelfth of a drop was quite sufficient to produce a decided effect, and at no time was it increased to more than one eighth, dissolved in alcohol and water. (Mr. R. Harrison, p. 41.)

Calabar Bean.—Dr. E. Watson reports on the use of Calabar bean in traumatic tetanus. He and two of his friends have treated six cases with it, and all terminated favourably. He considers that the Calabar bean produces the exact counter effect on the spinal marrow to the state in which tetanus originates. In its action it appears to resemble nicotine. (Dr. E. Watson, p. 43.)

In treating tetanus by Calabar bean we must be careful to begin the administration of it as early in the case as possible. If there is much difficulty in swallowing it should be injected hypodermically, as given in this manner it is both certain and rapid in its effects. Eight grains of the extract rubbed down in and mixed with an ounce of spirits of wine, form a solution or tincture every ten minims of which contain one sixth of a grain of the extract. (Dr. E. Watson, p. 48.)

Toothache.—*Carbolic Acid.*—To one drachm of collodium flexile (B. P. 1867) add two drachms of Calvert's carbolic acid, full strength. A gelatinous mass is precipitated. A small portion of this precipitate inserted into the cavity of an aching tooth gives immediate relief. (p. 53.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Acupressure.—The advantages of acupressure over the ligature consist in: 1, The ease of its application without the aid of an assistant. This is by no means to be despised in country practice. The spring artery forceps does not confer an equal advantage. 2, The control of the venous hemorrhage at the same time as the arterial. 3, The short time (forty-eight hours at the very longest) that a pin need be left in the wound in order to insure complete closure of the vessel. 4, The offensive moist seton, in the shape of the ligature hanging from the wound is gone, and this of itself is sufficient to recommend the use of the pin and wire. On the other hand it is not found in London that union by the first intention without the formation of a drop of pus occurs, as is the experience at Aberdeen. The author has employed acupressure in thirty-one cases, but not in one instance was there absence of pus. (Mr. C. Foster, p. 158).

The Aberdeen Twist.—What is known as the Aberdeen twist consists in passing the pin by the side of the bleeding vessel, and after it has pierced a small portion of tissues, bringing the

point out of the flesh, then turning the pin round a quarter of a circle, allowing its point to go over the vessel, and fixing the needle into the soft parts beyond. This method is very effectual and easy of application, and does not require the use of a wire. (Mr. C. Forster, p. 155.)

Arterial Hemorrhage.—*Dr. Taylor's Acupressor Arteriæ.*—Bleeding arteries may easily and effectually be closed by small prehensile forceps, which may be removed at pleasure with perfect ease, and so have all the advantages of acupressure without the disadvantage of compressing other tissues than the vessel itself. The instrument is made by bending a piece of steel wire in the middle upon itself so as to form one spiral turn, and then by crossing the two ends at a little distance from the spring, and adjusting them in length so that they form a small pair of forceps. One extremity should be bent upon itself, so as to give, with the single end which meets it, a firmer hold upon the vessel. A fine piece of wire should be attached to the spring of the forceps for its withdrawal, which is accomplished by passing a slender silver tube down the wire and upon the forceps, the latter losing its hold of the vessel immediately its arms are compressed by the tube. These forceps might be manufactured by the gross at a very cheap rate. (Dr. W. Taylor, p. 161.)

Dr. Wolfe's Canula Artery Forceps.—This form of canula forceps will be understood at once by reference to the woodcuts. They are intended for the arrest of hemorrhage, in the same way as by acupressure. (Dr. J. R. Wolfe, p. 164.)

[In the article at page 164 the references (*a. b.*) to the wood cuts are wrongly placed—they should be reversed.]

Closure of Arteries by Torsion.—Prof. Syme, of Edinburgh, states that since he has arrested arterial hemorrhage (except from the largest vessels) by torsion he has not had a single case of secondary hemorrhage, either public or private. In one case lately he trusted to torsion alone for closing the popliteal artery, but does not advise this as a rule. The late Mr. Teale, of Leeds, used torsion equally as much as Mr. Syme, and it was always his plan to twist the end of the vessel quite off. (Prof. Syme, Mr. T. P. Teale, p. 153.)

By torsion alone almost all arterial hemorrhage can be arrested. No ligatures are required even in amputation of the thigh. Syme's torsion forceps are the best. (Mr. E. Hamilton, p. 368.)

Belladonna as a Cardiac Tonic.—There is no medicine in the whole materia medica which at all approaches belladonna in its

simple, direct, immediate, and powerful influence in exalting the force and rapidity of the heart's action. In all conditions and diseases in which there is a depression of the sympathathetic nervous influence, such as syncope from asthenia, or shock, in the collapse of cholera, belladonna, or the subcutaneous use of from the hundredth to the fortieth of a grain of sulphate of atropia forms the most appropriate treatment. (Dr. J. Harley, p. 387.)

Detection of Minute Quantities of Blood by the Micro-Spectroscope.—By means of the micro-spectroscope, a combination as its name indicates of the spectroscope and microscope, as small a quantity of blood can be detected as the thousandth of a grain of the dried fluid. The dark absorption bands peculiar to old blood can by this means be distinctly seen. (Dr. W. B. Herapath, British Medical Journal, March 7, 1868, p. 217.)

Dilated Blood-Vessels.—*Aconite.*—Of those medicines (excepting direct astringents) which act upon the blood-vessels, cold and heat may be taken as the types. Cold produces great contraction of the vessels, lasting a long time, and is succeeded by reaction when the cold is removed. Heat produces slight contraction, which soon passes off, and is succeeded by decided dilatation of the blood-vessels. Thus it will be seen that the recognised action of cold is its primary action, whilst that of heat is its secondary action. The action of aconite is very similar to that of cold. Its primary effect is upon the heart, causing contraction of the blood-vessels which supply its tissue, and a consequent decrease of the vital properties of the organ. A state of collapse is produced, somewhat similar to that of cholera. But the further phenomena of the drug, which are equally important if not more so than those just named, appear to have been totally overlooked. A reaction occurs, and the blood-vessels of the heart become preternaturally dilated, the circulation more active, and a state of fever is present. In medicinal use we want, however, only the primary action, and for this purpose we must give it in very small doses, as of a quarter of a drop to a drop of the tincture, repeated every half hour or hour. Two or three doses will cure a feverish cold, and sometimes the relief is immediate. When a blood poison is present the effect is not so marked; nevertheless, in rheumatic fever, considerable relief is sometimes obtained. (Dr. Reith, p. 373.)

Hemorrhage from the Rectum.—If the blood comes before the motion, it probably arises from the lower part of the rectum. If it comes during the motion, it is probably caused

by the feculent matter running across the plugged-up mouth of a wounded vessel, and displacing the plug. If it comes after the motion, it is evidently associated with tumour or something of the sort higher up the intestine. It is necessary to ascertain accurately the source of the blood, and for this purpose the patient must have a stool in your presence, and be directed to bear down thoroughly. If necessary, an enema may be given to facilitate the action of the bowels. In nine cases out of ten you will see at once the source of the bleeding. The hemorrhage may come from a small depressed ulcer seated near the anus, or it may come from several little raspberry-like prominences. As they are everted during defæcation, it will be extremely easy to apply a ligature or the actual cautery or nitric acid to the bleeding part, according to your own choice. (Mr. J. Hilton, p. 144.)

Intermittent Pulse.—That which impresses me most in favour of the cerebral origin of intermittent pulse, is the mode in which it appears in the prime of life. I have never met with a case in which it has not been traceable to some form of cerebral excitement with succeeding depression—grief; shock from failures of enterprises in business; disappointment; violent outbursts of passion; remorse; degradation; and, most common of all in this madly striving age, overwork of brain. The disease is owing to a failure of the left ventricle to contract when full to the proper degree with arterial blood, it thus with a violent effort makes up for previous inaction. The disease is nervous, not connected with disease of the heart itself. Nor does it arise as is so often thought, from dyspepsia, although it is true that many dyspeptic persons have intermittent pulse. (Dr. B. W. Richardson, p. 66.)

Plugging the Posterior Nares.—To detach and withdraw the plug by which the posterior nares has been stopped is not always easy. It is best effected by passing the gum-elastic catheter, by the aid of which the operation is supposed to have been performed, along the string by which the plug has been drawn into its place, and the end of which must, until wanted, be fastened on the forehead by adhesive plaster. When the plug is reached by the catheter it can be detached by it and carried into the fauces, and so withdrawn. (Mr. J. I. Mackenzie, p. 152.)

Wounds of the Palmar Arch.—Arrest the circulation through the ulnar and radial arteries by acupressure. The pin may be removed in forty-eight hours. (Mr. C. Forster, p. 159.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

Hooping Cough.—The author states that he has found advantage, in cases of hooping cough, from evaporising and diffusing in the air of rooms occupied by the patients the vapour of carbolic acid. He has designed a simple apparatus for the purpose, which may be obtained of Mr. Salt, of Birmingham. (Mr. V. W. Blake, p. 91.)

Medicinal Inhalations.—Dr. Adams, of Glasgow, describes a very efficient apparatus which he has devised, by which a patient without any effort on his part may inhale a warm medicinal vapour for any length of time. It is compact and ready for use, and is so put together that there is no necessity for frequent and nice adjustments, consequently the risk of troublesome disarrangements, or of accidental injury of the instrument, is exceedingly small. It may be procured from Mr. P. Harrower, or Mr. D. P. Walker, chemists, Glasgow, price 5s. (Dr. J. Adams, p. 393.)

Scrofulous Pneumonia.—In this disease, which simulates as far as regards its physical signs various other diseases, the deposit occurs first at the base of one or both lungs, and seldom, at any period of the case occurs in the upper lobes. Crepitation as in ordinary pneumonia is the first physical sign which occurs, and it is very difficult to distinguish the case from one of pneumonia, but there are these differences: there is not the extreme weakness and prostration of pneumonia; the breathing is but little oppressed; the expectoration is not rusty, unless there has been decided hæmoptysis; and lastly, there is not in scrofulous pneumonia the dulness on percussion which occurs in ordinary pneumonia. Pneumonia begins to clear up after a definite time, whereas in scrofulous pneumonia day after day passes and yet the physical signs remain, still few and indeterminate, and consisting only of crepitation with after a time, a little dulness. This long continuance of such physical signs is quite sufficient of itself to lead us to a correct conclusion on the nature of the disease. The amount of elevation of temperature in scrofulous pneumonia (102°, 103°) is usually less than in ordinary pneumonia (104°, 105°). The crepitation of scrofulous pneumonia is generally a little larger than that of ordinary pneumonia, and often accompanies expiration as well as inspiration. As the physical signs in scrofulous pneumonia deepen in intensity, the fine rhonchus is heard over the whole lung, whereas in ordinary pneumonia it is only heard at the edge of the affected part, and is inaudible at that part where the disease first commenced. (Dr. S. Ringer, p. 79.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Disease of the Gums. — After washing the teeth night and morning with a soft tooth-brush, apply on the brush to the affected parts a little of the following lotion : Carbolic acid, one scruple ; rectified spirits of wine, two drachms ; distilled water, six ounces. (Mr. Hilditch, p. 169.)

Flatulent Distension of the Colon. — Creosote and carbolic acid are the most likely remedies to check flatulent distension of the colon from gaseous evolution ; and when there is also diarrhœa, carbolate of lime may be given with marked benefit. It may be obtained from Squire, of Oxford Street. (Dr. S. O. Habershon, p. 168.)

Gastro-Neuralgia. — *Arsenic.* — In cases of gastrodynia, in which the pain comes on at irregular intervals, and bears no relation to the time of meals, and especially when the source can be traced to mental anxiety, the remedy of most service is arsenic. Some interesting cases are related in illustration of this, in the two first of which the attributes of neuralgia were well marked, the pain occurred spontaneously, without inflammation, and was sudden in its coming and its going. In one case the pain was induced by food, but the failure of ordinary remedies for pain after food, and the fact of the case being one of phthisis, led to a trial of the arsenic, and this proved successful. There is a connection between ague and some cases of gastrodynia, and if we have a history of the previous existence of ague, along with the fact of the occurrence of the pain independently of the ingestion of food, arsenic is almost sure to cure the case. (Dr. A. Leared, p. 92.)

Dr. Norris, of Stourbridge, relates his own case as illustrative of the use of arsenic in neuralgia of the abdominal viscera. He had suffered from abdominal pain, coming on generally during digestion, and was in the habit of obtaining relief by a few drops of laudanum or a cigar. The pain left him under the use of arsenic, in doses of five or six drops twice a day. (Dr. Norris, p. 98.)

Inguinal Hernia. — *Position in the Reduction of.* — Direct the patient to stand up, and place yourself (also standing) behind him, and encircle his body with both your arms, grasping the tumour with both hands, and you will probably effect in a few moments what you would have failed to accomplish in as many hours with the patient lying down. This has been so successful that the author has never yet had a case which he could not reduce. Femoral hernia, he observes, cannot be reduced when the patient is erect. (Dr. J. B. Bond, of Yarmouth, Nova Scotia, p. 165.)

Obstinate Constipation.—*Faradization of the Bowel.*—Cases of habitual constipation are generally extremely difficult of cure, every remedy after a time losing its effect. In such cases faradisation of the bowel ought to be employed, as it offers a good prospect of producing permanent good effects. For this purpose an insulated metallic sound must be introduced into the rectum, a moistened conductor connected with the positive pole being placed on the abdomen. The top of the metallic sound only is free for about an inch, and at the other extremity it should be connected with the negative pole of the primary faradic current. The current, which should be of moderate power, may be allowed to pass for fifteen minutes at a time, and the process repeated twice a week. It is not well to discontinue aperients altogether at first. (Dr. J. Althaus, p. 166.)

Pain after Food.—*Black Oxide of Manganese.*—When there is gastric pain, worse after ingestion of food, a sense of heat, epigastric tenderness, and redness of the tip of the tongue, the purified black oxide of manganese is a superior remedy to bismuth. In a few cases, however, the irritability of the gastric mucous membrane is continued in that of the small intestines. The pain is then associated with a tendency to diarrhœa, and the astringency of bismuth becomes valuable. (Dr. A. Leared, p. 97.)

AFFECTIONS OF THE URINARY ORGANS.

Acute Nephritis and Chronic Albuminuria.—From its power of contracting dilated blood-vessels belladonna holds out prospect of being of use in the above-named affections. In chronic albuminuria, of course no benefit can be expected when fatty degeneration has commenced. (Dr. J. Harley, p. 389.)

Albumen in the Urine.—There is no better mode of testing for small quantities of albumen than that of allowing a small quantity of urine to trickle slowly down the side of a test-tube upon about half a drachm of fuming nitric acid. (Dr. A. Clark, p. 102.)

Diabetes.—Give half drachm doses of the etherial solution of peroxide of hydrogen, mixed in an ounce of distilled water three times a day. We possess in peroxide of hydrogen a ready means of destroying, by oxidation, the sugar in the blood, and of also maintaining the animal heat, which in the treatment of diabetes is an important consideration. What is sold by Mr. Robbins as Dr. Richardson's ozonic ether is in reality a solution of peroxide of hydrogen in ether. A most interesting

case is related showing the gratifying results following this treatment. Before commencing it the patient had been passing as much five quarts of urine each night, and the thirst was intense. The last report states that the quantity was reduced to twenty-four ounces, and that the thirst was quite relieved. (Dr. J. Day, Geelong, Australia, p. 100.)

ENLARGED PROSTATE.—One man in every three who dies above the age of fifty-five has some enlargement of the prostate, but only one in seven has any symptoms of it, and probably only one in ten requires treatment for it. The size of the prostate does not influence the degree of obstruction to the urethra; it is the amount of enlargement of the middle lobe which is the matter of import. For ordinary use in prostatic cases the English gum-elastic catheter is by far the best. The point is generally not curved as it should be. The instrument should be well curved to the very point: with this object it may be kept on an over-bent stylet for a month; it will thus pass easily without a stylet, for the tendency to over-curve which the instrument possesses will ride it over the obstruction into the bladder. The general treatment of prostatic enlargement consists mainly in preventing local congestion, the most frequent cause of which is the undertaking long journeys, and exposure to cold. (Sir H. Thompson, p. 195.)

It is a peculiarity in the frequency of micturition which exists in hypertrophy of the prostate, in common with all serious urinary diseases, that it is more urgent at night than during the day. This is an aid in diagnosis, and should be made a point of inquiry in investigating urinary diseases. (Sir H. Thompson, p. 170.)

Diagnosis between Prostatitis and Calculus.—Pain in micturition from prostatitis is towards the end of the act of micturition, when the bladder begins to contract on the tender prostate; but when the pain is from an inflamed bladder, it is at the commencement of the act of micturition. Pain from calculus in the bladder also occurs towards the end of micturition, when the rough surface of the stone comes in contact with the mucous membrane. Pain from this cause however is very characteristic, it is increased by movement and is felt very acutely about the base of the glans penis. Pain from prostatitis also sometimes occurs in the penis, but is less severe, and is not much increased by riding in a rough-going vehicle, as is that from stone. (Sir H. Thompson, p. 171.)

There is one point of great service in the diagnosis between prostatitis and calculus, especially if we wish to avoid the the passage of a sound. Tell the patient to pass a little urine

so as to wash out the urethra, set that aside in a vessel, and then let him empty the bladder into a second vessel. If the first only is thick from pus or mucus, whilst the second is clear, it is prostatitis; if the second is turbid, the cause of the turbidity is in the bladder, and may be stone. (Sir H. Thompson, p. 172.)

Frequency of Micturition.—Whenever the natural characters of the urine are altered before it reaches the bladder, the secretion produces irritation. Diluted or watery urine is often regarded as unirritating; on the contrary, it is not generally well retained by the bladder. In pyelitis, and in almost all organic changes of the kidney, in Bright's disease, and in diabetes, there is frequency of making water. The bladder is always irritated by the pale urine passed by hysterical patients. (Sir H. Thompson, p. 170.)

Precaution in Examining the Urine.—Always first flush the urethra and take the urine in a separate vessel, say a wine-glass, and let the rest of the urine be passed into another vessel. If this is not done we get any matter from prostatitis or urethritis mixed up with the whole quantity of urine. A case came under Sir H. Thompson's observation which had been treated by a physician for months as pyelitis, whereas when the urethra had been flushed the rest of the urine was quite healthy, proving that the pus came from some part of the urethra. (Sir H. Thompson, p. 172.)

Lithotomy and Lithotrity.—It may be affirmed as a maxim that between puberty and the age of fifty lithotrity is the most suitable and the selected operation. Even above the last-named age a majority of cases will be most suitably treated by this method; and if any between puberty and extreme old age be rejected or set aside for lithotomy, it must be for certain unfavourable conditions and indications which, if likely to increase the dangers of lithotrity, will most assuredly have a similar effect in lithotomy. In other terms, it may be said that all the cases of stone in the bladder in the adult, with complications, are now set aside for lithotomy; whilst all the simpler cases most promising of success are treated by lithotrity. (Sir W. Fergusson, p. 224.)

It is of the greatest importance to determine the nature and size of a stone; for now that we have two operations for its removal, if we crush the large stone and cut for the smaller one we shall have a greater mortality than if we performed lithotomy in all cases, as was formerly done. The rule may be accepted that all stones, under puberty, with very few exceptions, are to be cut, unless they are very small and can

be crushed, say, in one operation. All the cases above puberty are to be crushed, with certain rare exceptions. The first exception is in a case of oxalate-of-lime calculus, which is, let us say, an inch in diameter. Under an inch in diameter you may crush an oxalate-of-lime calculus. A stone which is two inches in diameter, either phosphatic or uric acid, had better be cut. No doubt a rather larger phosphatic stone may be crushed, for it is very friable. No doubt it is possible to crush any stone; but the number of sittings required and the amount of irritation produced, prevent its being done successfully. If there is a bad stricture of the urethra, or a diseased state of bladder, it is better not to crush. Hypertrophy of the prostate need not interfere with lithotrity, it is only a question of delicate manipulation. (Sir H. Thompson, p. 221.)

Lithotomy.—The internal incision is of more consequence than the external, although all are agreed that the latter should be free. For some years Sir Henry Thompson states that he has operated as recommended by Erichsen (after the original operation by Dupuytren), by incising both lobes of the prostate, in order to obtain room when the stone is large, and has found the plan satisfactory. (Sir H. Thompson, p. 234.)

Celsian Operation for Stone in the Bladder.—The oldest description of any lithotomy operation we have is that of Celsus, who made a semilunar incision, the concavity of which embraced the rectum. This plan of operating has been lately revived by the author. He commences the operation with what he styles "the famous, but antiquated, if not despised, semilunar incision of Celsus"; but as regards the deep parts of the wound and prostate, opens the bladder as in the ordinary lateral operation. The great advantage of this free external wound is the ease with which the finger can be introduced deeply into the wound, above the knuckle if necessary, whereas in the ordinary small incision the finger is arrested at the web between it and the second finger. There is also more room for the introduction of instruments, whereas when the ordinary lateral incision is made the finger seems quite surrounded by the edges of the wound, and the instruments are introduced with less ease, and consequently cannot be as satisfactorily used as when the external wound is more ample. The most frequent cause of failure in lithotomy is too short an external incision. (Sir W. Fergusson, p. 225.)

Mr. Erichsen, in an article drawn out by that from which we have taken the preceding synopsis, says that he quite agrees with Sir W. Fergusson in the adoption by him of the crescentic external incision after Dupuytren, but makes the

practical suggestion that it is better also to divide the prostate by a transverse incision, cutting into both lobes. This may be done with great certainty by means of a lithotome caché, which cuts as it is withdrawn. The advantage of this is that a large internal incision can be made if necessary without risking cutting beyond the prostate. The use of the rectangular staff is recommended in preference to the ordinary curved staff, whatever operation is performed. (Mr. J. E. Erichsen, p. 238.)

Medio-Bilateral Lithotomy.—Mr. Jackson, of Wolverhampton, uses a broad straight gorget, with a bilateral cutting edge, for the purpose of dividing the prostate bilaterally. This gorget terminates in a probe point, which is lodged in the deeply grooved curved staff previously introduced into the bladder. During the division of the prostate the fore-finger of the left hand is placed firmly in front of the prostate. (Mr. V. Jackson, p. 235.)

Mr. Hutchinson bears testimony to the advantages of median lithotomy with bilateral section of the prostate. The knife, or rather gorget, he uses, is similar to that of Mr. Jackson, but he uses a rectangular staff which, at the end of the groove has a little thimble to receive and retain the probe point of the gorget. Instead of making the gorget move on a staff which is fixed, he passes the probe end of the former forwards into the thimble, and having thus locked them both firmly together, he then takes the staff and passes both together forwards and upwards into the bladder. The ease and certainty with which an operation of this kind is accomplished, together with the certainty of avoidance of injury to important parts recommend it to general adoption. (Mr. J. Hutchinson, p. 236.)

Sounding for Stone in the Bladder.—The instrument used for sounding for stone should have a small, short, curved beak, because it can be turned in every direction. It is very easy indeed to miss a small stone when an instrument with a large curve like a catheter is taken, indeed it is probable that more stones were missed with such instruments than were found. A big stone can be found with any sound, but the great object is to find the small stones. The sound may be provided with a little slide on the shaft, which by proper manipulation enables you to ascertain very nearly the size of a stone. A phosphatic calculus when in the bladder gives a dull note when struck; whereas the uric acid one gives a hard ring. To determine if there are more stones than one in the bladder seize one with a lithotrite, and then move it about as a sound for others. (Sir H. Thompson, p. 219.)

Stricture of the Urethra.—*Diagnosis.*—When a case of stricture, or supposed stricture, is first examined, a fair sized instrument must be first taken, say No. 8 or 9. If there is a stricture you ascertain its position but do not expect the instrument to pass, whereas if there is no stricture it passes at once. When the instrument is passed through the first few inches of the urethra, it must be kept well on the floor of the canal in order to avoid the lacuna magna. The second place in which a mistake may be made is at the commencement of the membranous portion of the urethra, or rather, in the bulb, where the canal is wider and more distensible than in the membranous portion. It is the lower part of the bulb which bags out and is so distensible, and consequently it is here that the point of the instrument is liable to catch, and, if force is used, a false passage to be made; consequently the rule in this part is to use an instrument with the point pretty well turned up and to keep it on the roof instead of on the floor of the passage. The last place at which there may be difficulty in passing an instrument is at the neck of the bladder. We used to hear a good deal about "stricture at the neck of the bladder"; there is no such thing. It is simply because there is sometimes difficulty in passing the neck of the bladder that it came to be regarded as a locality of stricture. In this case also a well curved instrument is the best thing to go in. (Sir H. Thompson, p. 181.)

There is no such thing as "impermeable" stricture. It is a contradiction in terms. Stricture is a *narrowing* of the canal; it is not an obliteration of it. A stricture always admits urine, more or less in quantity, to pass through it, and whenever urine passes outwards through a stricture, an instrument ought with care and perseverance to be got in. (Sir H. Thompson, p. 177.)

Flexible versus Solid Instruments for Dilatation of Stricture.—Flexible instruments are very much preferable to solid ones for dilating a stricture. They pass more easily, and consequently with less pain, and will insinuate themselves where a solid instrument would fail. This latter property is especially seen in the French instruments, which are so flexible that you can wind them round the finger, and possess a tapering point slightly bulbed at the end. This instrument passes very readily, almost without your knowing it, and it requires no knowledge of anatomy to use it. (Sir H. Thompson, p. 185.)

To pass a Bougie through a Narrow and Tortuous Stricture.—Do not rely upon mere groping to find the orifice. You should always adopt some method—any method you please, provided that it shall seem to you exhaustive of the different modes of

exploring the urethra. Take a small instrument and slide it down first the roof of the canal because that is the firmest part, and by following it you are most likely to carry the point in. The floor on the contrary, is the softest, loosest, and most spongy part, and will be most likely to yield to the instrument and give way. If your first effort does not succeed, take the right side ; if that does not do, take the left ; if that does not do, take the floor. There is no other method so calculated to help through a difficult stricture. If you are very careful, you may make the attempt in this manner for twenty or thirty minutes without doing any damage. (Sir H. Thompson, p. 180.)

Injection of Oil in Stricture.—When you have a very narrow stricture to deal with, it is sometimes of use to throw half an ounce or an ounce of olive oil into the urethra, holding the meatus well round the syringe. By this plan the passage is distended, and you may sometimes succeed in passing an instrument when you have been unable in any other way. The oil must not be allowed to escape during the introduction of the catheter, and the plan must not be tried at all if there is a false passage. (Sir H. Thompson, p. 190.)

Stricture of the Urethra with False Passage.—When you have a case of stricture with false passage to treat, remember that the false passage commences usually on the floor, and generally at the bulbous portion of the urethra. When the instrument is in the false passage you can tell at once that it is so by introducing the finger into the rectum when only the thin coats of the bowel will be felt between the finger and the instrument. If the instrument is in the right passage, you feel the whole thickness of the prostate, not always very considerable, between it and your finger, still quite enough to show that you are in the right path. If you find the catheter goes into a false passage withdraw it an inch or two, and in passing it again keep as close along the upper part of the urethra as you can, ascertaining by means of the finger that the instrument is not passing into the old route. (Sir H. Thompson, p. 189.)

Fastening the Catheter in.—Having succeeded in introducing a catheter with great difficulty, should it be removed or left in? It should be kept in for two or three days before removing it, and, if a metal instrument be used, it may then be exchanged for a gum-elastic one. (Sir H. Thompson, p. 191.)

Over-Dilatation of the Urethra.—It is certain from observations made by injecting the bladder and urethra with wax, that even in subjects of large size, the portion of the urethra begirt

by the triangular ligament rarely exceeds the dimensions of a No. 10 catheter. To dilate, therefore, beyond No. 10 or 11, is to dilate beyond the normal calibre of the canal; the urethra is kept in a state of irritation by this, and generally resents such treatment. (Dr. R. M. M'Donnell, p. 217.)

Contractile Stricture.—How are we to treat a stricture which after dilatation recontracts to its original size? In one of three ways. We may rupture, over-distend, or cut the unyielding and contractile fibres which constitute the stricture. Urethrotomy is perhaps the most generally employed, and internal oftener than external urethrotomy. But the rule is generally this, that the nearer the stricture to the meatus the safer and more useful is it to cut; the deeper seated the stricture the more desirable is it to over-distend. In performing internal urethrotomy the bistourie-caché must be passed half-an-inch beyond the stricture, and, the knife being unsheathed to the desired extent, the stricture is divided by drawing the instrument forwards. After this a catheter must be tied in for forty-eight hours, and after that an instrument passed every third day, then every week, and so on. The plan of over-distension is to be preferred to that of rupture in those cases in which it is not thought advisable to divide the stricture, and for this purpose the instrument devised by the writer answers every end. It consists of two blades separable to any extent at one part by means of a screw in the handle. This part of the instrument is placed in the stricture. Strictures situated in the bulbous part of the urethra are more suitably treated by over-distension than by cutting. (Sir H. Thompson, p. 192.)

Use of Caustics in Contractile Stricture.—Sir Henry Thompson, than whom there is no greater authority on subjects of this nature, condemns entirely the use of potash and other caustics in cases of contractile stricture. He believes them to be unnecessary, undesirable, and dangerous. (p. 191.)

Mr. Le Gros Clark states that for many years he has been in the habit of employing potassa fusa in the treatment of old and impenetrable strictures, with the greatest advantage, and without in a single instance causing dangerous consequences. A suitable instrument, with care in using it, together with a little patience, are requisite; but these are elements of success in every operation. (p. 211.)

Retention of Urine from Organic Stricture.—First try to pass the finest gum-elastic catheter, and if successful tie it in; if it cannot be passed try a No. 1 silver catheter. Efforts to pass instruments should not be persevered in too long, and great

care must be exercised that no injury is done to the urethra. If unsuccessful put the patient into a warm bed, apply hot fomentations to the parts, and give opium very freely so as to relieve the involuntary straining efforts which make the case worse, and afford no relief. By this means the dribbling of urine will probably become more free, and in two or three days it will be found that a catheter can be passed. If, however, these means fail, and a greatly distended bladder be felt above the pubes, we are necessitated to puncture the bladder. The plan of opening the urethra behind the stricture has been now nearly abandoned, as it is extremely difficult to hit that passage, and a cutting operation at all generally becomes unnecessary if the urethra can be allowed to lie fallow for a few days. Mr. Cock, of Guy's Hospital, has had more experience than anyone else in puncturing the bladder from the rectum. The finger of the left hand must be passed beyond the prostate until it reaches the bladder behind it, and the trochar glided along the finger to the point determined to puncture. This point is ascertained best by fluctuation being very distinct when transmitted from above the pubes by the other hand placed on the abdomen. A catheter can generally be passed a few days afterwards, and the stricture permanently cured. (Sir H. Thompson, p. 205.)

Retention of Urine in a young and healthy man without Stricture.—The history of such cases generally is that the retention came on in consequence of the patient sitting on a cold stone or damp grass when overheated by exertion, or of his indulging in some emotional excitement. He will also tell you that he has had gonorrhœa at some recent period. The cause is inflammatory swelling of the prostate; its condition resembling that of the tonsils in inflammatory sore throat. The old treatment was bleeding and the warm bath, this however is bad, and must not be relied on to procure relief. The best plan is to take a moderate sized gum catheter (one not larger than a No. 6, as a large one gives in these circumstances unneccessary pain) which has been kept for some time on a stylet, which has been overbent. In this manner there is generally no difficulty in relieving the patient. (Sir H. Thompson, p. 204.)

Vulcanised India-Rubber Catheters.—In cases of retention of urine without stricture, in old men, vulcanised india-rubber sounds are preferable to the ordinary catheters. They are so flexible and supple that they seem to find their own way, and catheterism, so to speak, performs itself, even in cases where there exists considerable enlargement of the prostate. (M. Richet, p. 219.)

Extravasation of Urine.—What happens when extravasation of urine occurs from rupture of the urethra behind a stricture? From the anatomical disposition of the fascia, the urine passes into the scrotum, up into the groin above Poupart's ligament, and towards the belly. The urine cannot pass backwards behind the scrotum neither can it get into the thighs. It does occasionally get as high as the chest. In such cases do not be afraid of the knife. On each side of the perineum make a good deep incision, which need not be limited to two or even three inches, because you are cutting into urine and not flesh. The incisions generally bleed rather freely. An incision should be made on each side of the penis, because if it is made in the middle line there is not sufficient communication for the incisions on the one side to relieve the other. When the bladder has been relieved in this way the urine continues to drain off by the incisions, and the stricture begins to improve so that in three or four days a catheter can be passed. (Sir H. Thompson, p. 208.)

Simple Urinary Fistula.—Whatever part of the canal a simple urinary fistula is connected with it almost always heals if the stricture is dilated, and no other treatment is required. Sometimes the external openings are numerous and the intervening tissue much indurated. These cases generally improve a good deal by dilating the stricture well, but sometimes they are not cured by that. The external openings of the fistula should then be well enlarged so that the urine may not be detained in the parts any longer than necessary. It may be necessary to excite inflammation in the track of the fistula by a hot wire, or by touching it with nitrate of silver. If the case is not cured by these means the patient must be taught to pass a catheter for himself, and told to use it every time he makes water. The catheter must not be tied in, for the urine by capillary attraction always passes between the outside of the instrument and the wall of the urethra, and so reaches the fistulous opening. (Sir H. Thompson, p. 210.)

Suppression of Urine.—*Belladonna.*—Belladonna will probably prove a remedy of value in cases of suppression of urine, whether accompanied with uræmic symptoms or not. As both the sluggish circulation and the torpid kidney are simultaneously aroused by the medicine, there is ground for expecting a restoration of the renal secretion. (Dr. J. Harley, p. 388.)

External use of Digitalis.—When fresh digitalis leaves, bruised, are applied as a hot poultice the digitalis is rapidly absorbed, so much so as to reduce the rapidity of the pulse by twenty

beats a minute, and sometimes more. This renders it a remedy of great value in cases of suppression of urine. In these cases the pressure on the renal circulation is relieved, and the urinary secretion rapidly re-established. Some very interesting cases illustrating this are related. (Mr. J. D. Brown, p. 241.)

Urethral Discharges.—If a patient has a urethral discharge, excluding gonorrhœa, such discharge is generally from the prostate, where it may be connected with prostatic calculus. But Cowper's gland or duct may be the seat of abscess, which is to be felt in the perineum, or an abscess in the walls of the urethra or deep in the perineum may cause pus to escape from the urethra. Such discharge, however, is most commonly from the prostate where it may be connected with prostatic calculus; if so, the prostate will be found tender on pressure, and by pressing afterwards on the urethra a little of the discharge may be worked up to the meatus and obtained for microscopic examination. The presence of a discharge of this kind causes a little albumen to exist in the urine, but no mistake in diagnosis should be made on this account. (Mr. J. Hilton, p. 146.)

Urethral Hemorrhage.—Blood from the urethra never enters the bladder; consequently blood in the bladder signifies hemorrhage from the kidney or bladder itself. (Mr. J. Hilton, p. 144.)

Urinary Pigments.—If healthy urine be allowed to trickle drop by drop, down the side of a test-tube containing about half a drachm of fuming nitric acid, and inclined at an angle of from 50 to 60 degrees, a yellow ring will form at the line of contact of the two fluids, and over this a narrow ring of an almost ruby-red colour. When bile is present there is also a green ring, and in certain morbid states of the system there is a purplish, bluish, or absolutely blue ring. In nearly every case of tropical dysentery a decidedly blue ring is produced so long as the disease is progressive, and the ruby-red ring reappears when the patient begins to recover. In any case where a blue ring exists it may be taken for granted that the case is taking a downward course. The depth of colour may be taken as an indication of the amount and rapidity of the destruction of the colouring matter of the blood, in short of the amount of waste going on. (Dr. H. Veale, p. 101.)

FRACTURES, DISLOCATIONS, OPERATIONS, AND DISEASES OF BONES AND JOINTS.

Antiseptic Principle of Treatment in Surgery.—The preparations of carbolic acid in use in surgery may be said to be three, carbolic oil, carbolic lotion, and carbolic paste. The composition of the first is carbolic acid and boiled linseed or other fixed oil, in the proportion of one to five; that of the second, carbolic acid and water in the proportion of one to thirty; and that of the third, carbolic oil with whitening, in the proportions requisite for the consistence of soft putty. The results of the employment of these preparations in surgery are almost to be called wonderful, and promise to remove and prevent an immense amount of human suffering. In a case of removal of a parotid tumour the wound was sponged with carbolic lotion, the edges stitched together with silver sutures and a plaster of carbolic paste applied. On the third day the wound was quite healed. In a case where the knee-joint was laid open by a scythe, the wound was freely sponged out with carbolic oil, the edges brought together, and a pledget of lint soaked in the oil laid over the wound. On the fourth day the wound was perfectly healed. Cases of compound fracture, psoas and mammary abscess, are treated most successfully by the same plan, which promises to create quite a revolution in surgery. (Prof. Syme, p. 119.)

Antiseptic Applications to Wounds.—It is probable that further experience will decide that it is unnecessary to apply carbolic acid in caustic strength to the interior of wounds; and that it will be found that the use of a solution of the acid in water or oil of sufficient power to act simply as an antiseptic, will prove sufficient for the purpose intended. (Mr. W. Adams, p. 123.)

Dislocation of the Hip Backwards.—*Reduction by Manipulation.*—The way to reduce dislocation of the hip backwards is not by abducting or rolling the limb outwards as has hitherto been practised, for if this is done the head of the bone is almost certain to roll under the acetabulum to its inner side. A better plan is the following: Bend the thigh on the abdomen, and then slowly move the limb into a straight line with the body, so that the head of the bone can be felt projecting in the buttock. It will be seen by examining the skeleton that the head of the bone is now in the groove above the outer side of the tuber ischii. Here it is opposite the least prominent part of the lower edge of the acetabulum, and if the femur is depressed whilst in this position the head

easily slips into the socket. To effect this, draw the limb forward from the abdomen, and then force it downwards or extend it. (Mr. G. W. Callender, p. 108.)

FRACTURE OF THE THIGH.—*Continuous Extension.*—The author describes fully the plan of treating fracture of the thigh by continuous extension by means of a weight and pulley. He says, "I have treated fractures of the thigh in all the various sites of fractures, and the result has been uniformly satisfactory." (Mr. L. Tait, p. 105.)

FRACTURE OF THE PATELLA.—*Treated by a Ring.*—Take a measure of the sound patella and have a ring made of iron (allowing for padding), which pad well with cotton wadding cut in strips and wrapped round the ring, over which a bandage can be applied. To each side of the ring sew strips of bandage. Then place a well-padded splint to the posterior aspect of the leg and thigh, which secure by a few turns of bandage at the lower and upper ends, the bandage being loose so as not to interfere with the circulation. Next bring the two fragments of bone into apposition, place the ring around the patella, and tie the strips of bandage over the splint, thus securely holding the ring in its place, and keeping the broken bone always in complete apposition, thereby giving the greatest possible chance for a bony union. (Dr. W. A. Gibson, Dr. P. Eve, p. 107.)

HARELIP.—By passing two or more curved needles, armed with silk sutures, from the raw edges immediately *under* the skin, carrying them through the whole thickness of the lip and tying them *inside*, we may effectually close the wound and run no risk whatever of those unsightly scars and marks which so generally result from the usual mode of applying harelip pins or sutures externally. (Mr. A. Duke, p. 143.)

At what Age should an Operation be undertaken for the Cure of Harelip?—The author considers that the age of earliest infancy should be avoided, but that the operation should be performed before the period of dentition. Even when a child cannot suck on account of the deformity, it is better to feed it with the spoon for a short time, until the parts have become larger and more fleshy. Again, although the mortality from this operation is very small, yet it is less in children who are a few months old than in those newly born, and again, many infants with harelip die from causes unconnected with this deformity during the first few weeks of life, thus dispensing with the necessity of an operation. On this question Sir W. Fergusson, than whom no one has a larger experience, thus expresses himself: "The earlier the operation is performed the better; assuredly

before teething. I decidedly prefer before the end of the first month in a healthy child, though the operation may be performed any time earlier to within a few hours of birth." One advantage of an early operation is that the child does not get into the habit of thrusting the tongue into the hiatus between the bones. (Mr. T. Smith, p. 139.)

Penetrating Wounds of Joints.—The principles upon which these serious injuries must be treated are as follows: the immediate closure of the wound, the adoption of perfect repose of the body, and immobility of the injured member, the local application of ice, and a moderate but nourishing diet. (Mr. J. Birkett, p. 112.)

Tapping with the Aid of Siphon-Power.—This may be easily applied in the following manner. Have a short piece of tube soldered on to the head of the canula about to be used, this piece being furnished on the outside with a screw; then let a similar piece, screwed so as to fit on to the screw of the canula, be inserted into the end of a piece of india-rubber piping about four feet long. This tube before the operation must be filled with water, and the contents retained by tying a knot on the extremity. When the trochar and the canula have been introduced and the trochar withdrawn, the tube being attached to the canula, the weight of the column of water contained in it will considerably facilitate the withdrawal of the fluid from the body. (Dr. W. Roberts, p. 138.)

Tumours.—*Treatment by Electrolysis.*—The electrolytic treatment of tumours, which is now upon its trial, acts in a threefold manner, viz., first, through mechanical disintegration of the tissues by the nascent hydrogen; second, through chemical destruction by free alkalies, potash, soda, and lime, which are evolved at the negative pole of the battery; and lastly, through a modification of nutrition. The positive pole is never to be introduced into the tumour operated upon, but always placed on the adjacent skin, whilst the negative pole, which should terminate in a needle or number of needles, is introduced into the interior of the tumour. The action of the electricity is thus upon the internal parts of the tumour, and all the risk of surgical operations is done away with. When the tumour is large the current should be passed through it from fifteen to thirty minutes at a time and repeated every day. For smaller tumours a less time will suffice. The results so far have been very encouraging. Nævus, bronchocele, and other innocent tumours have been destroyed, or rather, wasted with success. The results in the treatment of malignant disease are of course not so satisfactory, but

more experience is still required on this point. A battery suitable for the purpose may be obtained from Weiss, weighing 36 pounds, in a neat case, and only requiring a little attention every three or four months. (Dr. J. Althaus, p. 133.)

Dr. Althaus has now treated fifty-two non-malignant and eleven malignant tumours by electrolysis. He finds bronchocele, nævus, and the allied forms of tumour more favourable for this treatment than lipoma, enchondroma, and osseous tumours. Nævus is very successfully treated in this way, and in cases where the situation, size, or state of health of the patient counterindicate other plans of operation, this is especially called for. A case of bronchocele of large size, which many surgeons of eminence had pronounced incurable, is shrinking fast under the influence of the electrolytic treatment. The result of this treatment in cancer is not favourable. (Dr. J. Althaus, p. 133.)

Dr. Collis, of Dublin, does not use electricity for a short time daily, as recommended by Dr. Althaus, for the purpose of destroying tumours, but for lengthened periods. He employs a weak current, generated by batteries made of small cylinders or plates of wood covered with felt, and wrapped round with zinc and copper wire. These may be excited by salt and water, and carried about with the patient, tied up in gutta-percha or oiled silk. The tumour should be covered with a plate of zinc or copper which must be connected with the positive pole of the battery, whilst the negative pole is placed on the skin in some other part of the body. (Dr. M. H. Collis, p. 136.)

Vertical Amputation of a Finger.—In cases of vertical amputation of a finger by accident from machinery or otherwise, the healing may be much expedited by the following plan. Pass a strip of adhesive plaster firmly round the stump, having four loops of wire so placed round it that two of the loops shall be on the dorsal, and two on the palmar aspect, and having their eight free ends projecting beyond the stump. Then apply a larger piece of plaster over the other so as to give firmness, and twist the ends of the wires together so as to draw the skin forwards and partly cover the wound. (Dr. S. W. Poole, p. 289.)

Wounds.—*Dr. Richardson's Styptic Colloid.*—I have now, for more than a year, used Dr. Richardson's colloid styptic in a large number of cases of incised and lacerated wounds, some of formidable dimensions, with complete success in a large proportion of cases. In two-thirds of the cases so treated, I can with confidence assert that union by the first intention

has been obtained ; or that the reparative process has proceeded either without suppuration, even in bad cases, or with the suppurative process reduced to a very insignificant amount, and in no instance have I seen any injurious effects. (Mr. W. Adams, p. 129.)

It appears to me, from the wounds I have treated with it, to be particularly applicable to clean-cut wounds where the surfaces can be brought accurately together ; but the styptic is certainly often successful in obtaining union without suppuration in lacerated and often contused wounds, checking at all times, and often preventing suppuration in a wound which otherwise treated would be nearly sure to suppurate. (Mr. P. Hopgood, p. 130.)

When all is solidified, the dressing becomes a concrete, having a true organic hold or basis on the tissues, and rendering the wound almost as good as subcutaneous. (Dr. B. W. Richardson, p. 128.)

AFFECTIONS OF THE SKIN, ETC.

Acne.—The following ointment is serviceable in all the forms of acne. Washed lard, 50 parts ; sublimed sulphur, tannin, of each four parts ; laurel-water five parts. The proportions of sulphur and tannin may be increased according to circumstances, to six or eight parts. (Dr. Rodet, p. 281.)

Burns.—*Richardson's Styptic Colloid.*—Burns, which are both troublesome, offensive, and painful in the dressing, are by the use of the "colloid" deprived of half their terrors. When profuse suppuration sets in, if painted freely over the surface it at once removes the fœtor, protects the granulations, and the wound speedily heals. The first dressing is painful but not the others ; with moderate sized wounds the first dressing is also the last. Nothing further is required but daily painting with the "colloid." (Dr. J. Lowe, p. 12.)

Carbolic Acid in Scalds and Burns.—Mix carbolic acid with the ordinary oil and lime water emulsion in the proportion of one part to thirty. Linen rags should be saturated in this and spread on the scalded parts. These should be kept moist by smearing them over frequently with a feather dipped in the emulsion. The whole should be covered with oiled silk or gutta-percha tissue. (Dr. A. Wilson, p. 287.)

Apply closely over the whole surface of the burn two folds of lint dipped in a liniment, of one part of carbolic acid to six of olive oil. A double layer of tinfoil should be placed over

the lint, and the whole secured by means of a bandage. The effects so far as they can be told from one case related by the author, are very remarkable—viz., complete cessation of pain, and healing without the formation of any pus. (Prof. Pirrie, p. 286.)

Cystic Bronchocele.—The diagnosis of cystic bronchocele from the cysts formed in the areolar tissue of the neck, called "hydrocele of the neck" is easy. Tell the patient to swallow, and if it is bronchocele it will move up and down with the windpipe; if hydrocele of the neck, it will remain stationary. (Mr. T. B. Curling, p. 300.)

The cure of cystic bronchocele may be effected in three ways; by excision, seton, and injection. The real question however lies between the two latter. When iodine is used, about a drachm or a drachm and a half according to the size of the cyst may be thrown in and left there. This acts very satisfactorily in some cases, but is not so well adapted as the seton to the cure of cases where the cyst is much thickened. There is another advantage in the seton over injections—that it is free from the risk attending the rapid enlargement of the cyst, and the confinement of matter which has been known to prove fatal, as there is always an outlet for the escape of fluids. Upon the whole, the seton is the more valuable plan of treatment and the one most generally applicable to these cases. (Mr. T. B. Curling, p. 301.)

Foul and Chronic Ulcers.—*Richardson's Styptic Colloid.*—In old chronic ulcers, foul smelling and filthy, and whether syphilitic or not, the odour may be at once destroyed and healthy granulations established by the use of Richardson's styptic colloid. (Dr. J. Lowe, p. 15.)

Lupus.—The only internal remedies of undoubted value in lupus are arsenic and calomel in lupus of the face, and iodide of potassium in lupus of the limbs. Cod-liver oil, as also chloride of barium, produce often a degree of improvement but it is uncertain. Among remedies of no value we may rank steel, quinine, mineral acids, bitters, sedatives, change of air, and sea-bathing. With very few exceptions the external applications recommended in lupus are useless and impracticable. For instance, soothing or cooling dressings, and ointments of every kind are perfectly useless. The effects of iodide of potassium in lupoid ulceration of the limbs is very marked, a drachm weekly is generally sufficient. No remedy so rapidly checks quickly spreading lupus as calomel; no harm ever ensues from its proper employment,

and it should not be given so as to produce any action on the gums. As a local application perhaps no remedy is more useful than the acid nitrate of mercury. (Mr. J. L. Milton, p. 274.)

PRURIGO.—*Arsenic.*—The great internal remedy in prurigo is arsenic. Its value is chiefly seen in severe and long standing cases. Strychnia in doses of the sixtieth of a grain, and and cod-liver oil are of use, as is also the Turkish bath when the patient is not unsuited for it by feeble heart or general debility. (Mr. J. L. Milton, p. 290.)

Bromide of Ammonium.—The author relates a case of prurigo in an elderly woman, which she attributed to mental anxiety and bad food. Varies remedies had been tried without success, including the subcutaneous injection of morphia. Bromide of ammonium in doses of ten grains three times a day was then admininistered, and gradually increased to twenty. The symptoms gradually disappeared after this, and a perfect cure resulted in four to five months, although the disease had existed six years. (Dr. H. Purdon, p. 296.)

SMALL-POX.—*Carbolic Acid to prevent Pitting.*—The author recommends the application over the face of an ointment made of carbolic acid, two drachms; mutton suet, two ounces; and coloured with lamp black. This should be spread thickly upon black cotton wadding, and applied over the face and forehead, holes being cut for eyes, nostrils, and mouth. This mask should be changed every second day, and the face gently washed with soap and warm water. In a case related, this quite prevented pitting, and the suppuration was much diminished in amount and duration. (Dr. H. Yates, of Kingston, Canada, p. 285.)

WARTS AND CANCER.—There seems to be truth in the opinion that there is some relationship between warts and epithelial cancer. The similarity between the two at their commencement, and the frequent occurrence of them in the same individual, both confirm the idea. The application of the acid nitrate of mercury is the best way of getting rid of warts. (Mr. J. Hutchinson, p. 302.)

VENEREAL AFFECTIONS.

BUBO.—In the treatment of threatened bubo every endeavour should be made to prevent suppuration altogether, or to put it off as long as possible, and to confine it within the narrowest limits. If any matter forms it should be evacuated at once

by free incision, as this often prevents the necessity of subsequently opening troublesome sinuses. (Mr. Durham, p. 310.)

Syphilis.—*Iodide of Potassium.*—Iodide of potassium in large doses (*i.e.*, from thirty to seventy-five grains daily) is almost a specific for the cure of large spreading tertiary, or late secondary, ulcerations of the skin, such as those so common and generally so intractable, affecting all parts of the body, and which often follow an eruption of rupia. For those late syphilitic eruptions which affect the nose so disastrously, and often so rapidly, these same doses are equally valuable. Iodism is exceedingly rare in presence of tertiary syphilis; and large doses do not, as a rule, occasion loss of flesh. If they cure the syphilis, the patient usually gains weight under their influence. The iodide of potassium is however as undesirable in primary syphilis and in early secondary symptoms, as the iodide of mercury in *small* doses doses is valuable at that period of the malady. (Sir H. Thompson, p. 312.)

Iodide of Potassium with Iron.—It will be constantly found that iodide of potassium given in small or moderate doses, fails to arrest the ravages of ulcerative secondary or of tertiary syphilis. There is, however, a singular increase in the efficacy of this drug, produced by combining it with a salt of iron, or better still, a double salt of iron and quinine. In this combination it will rarely fail to stay the progess of the disease. (Dr. P. Eade, p. 313.)

AFFECTIONS OF THE EYE AND EAR.

Cataract.—*Extraction by Linear Flap.*—The patient being completely narcotised, the lids kept apart by the stop speculum, and the eye fixed with a pair of forceps, enter a Von Gräfe's cataract knife about the junction of the middle with the upper third of cornea, bring out the point at a counter-puncture similarly placed; the knife must be held parallel to the iris, and be carried upwards about two lines and a half, then turned forwards and the section completed by a gentle sawing movement, thus forming a very small elongated flap, situated well back in dense tissue with sloping sides and transverse centre, occupying the summit of the true cornea. The above is an outline of the mode of procedure in this operation, which is said to be more successful than the ordinary modes of extraction. (Dr. C. Taylor, p. 244.)

Linear Extraction of Cataract.—After lacerating the capsule, the scoop should be passed so as slightly to draw open the pupil, and then pressed with its edge against the outer margin of the lens, which immediately tilts over on its axis, and the scoop being thrust onwards, the cataract comes to lie in the hollow of the instrument, and may then be withdrawn from the eye. The scoop must never be passed behind the lens as it lies imbedded in the vitreous. It is a decided advantage to make the incision in the cornea well forwards; if it is made too near the sclerotic, there is much greater chance of prolapse of the iris and escape of the vitreous. The usual curved scoop is the best, but it is an advantage to have its end serrated, and the curve well fenestrated, as the lens fits more firmly into this open space, than it does with an ordinary spoon-shaped scoop. (Mr. C. Macnamara, p. 254.)

New Operation for Cataract.—That operation is the best which the most diminishes the risk of primary corneal suppuration and iritis. These objects are gained by a small size of the corneal wound, and the removal of the lens in its unopened capsule. The operation is performed as follows:—The patient is kept throughout the operation fully under the influence of chloroform, so fully indeed, that he shows no sign of reaction when the conjunctiva of the eye which is not to be operated on is seized with forceps. A small incision is then made at the corneo-sclerotic junction, the adjacent portion of iris is excised, the zonula is opened with a small hook close to the wound, the scoop is gently passed into the vitreous cavity just behind the lens, and the latter is slowly removed. The after-treatment may be much as after extractions; the eye may, however, be safely examined at a much earlier period, say 24 to 36 hours after the operation. (Mr. T. Windsor, p. 252.)

New Auriscope and Endoscope Combined.—Mr. J. Hinton, of Guy's Hospital, recommends an instrument which is handy, of moderate cost, and combines both an auriscope and endoscope. It has also the peculiar advantage of enabling two persons to see at the same time. A woodcut and description will be found at p. 261.

Removal of Foreign Bodies from the Ear.—Let the surgeon take six inches of fine wire and double into a loop; then, having the patient placed on his side, pass the loop into the ear as far as it will go, and turn it a little gently. At the first or second withdrawal the foreign body will come out in the loop. The wire being flexible gives no pain, and cannot possibly do damage. (Mr. J. Hutchinson, p. 262.)

MIDWIFERY, ETC.

Cancer of the Breast.—Neuralgic pains down the arms is a symptom of great value in determining the nature of a tumour of the breast; for it is rarely present in any other affection of that organ than the cancerous. It indicates the probability that the deep cervical or axillary glands are implicated in the disease, and are pressing on the nerves, causing pain, although these glands may not be felt. (Mr. T. Bryant, p. 364.)

Dr. Richardson's styptic colloid is an excellent application to ulcerating cancer of the breast. The horrible effluvium from the wound ceases and healthy granulations spring up. The wound may even close up altogether, and the enlarged axillary glands disappear. A very interesting case in illustration of this is given. (Dr. J. Lowe, p. 14.)

Cancer of the Uterus.—Dr. Atlee, of Philadelphia, has great faith in the internal use of arsenic in cases of cancer of the uterus, combined with the local use of a very strong solution of iodine in glycerine. (Mr. T. S. Wells, p. 364.)

Deodorized Sponge Tents.—Let the tent be made upon a silvered knitting-needle as an axis, and after it is finished and the needle removed, stop up one end of the central canal with a drop of wax. Now carefully fill the canal with powdered permanganate of potash, and close the other end with wax. It is then ready for use, and on removal no unpleasant odour can be detected. It is well to keep tents of different and regular sizes, properly numbered according to the size. (Dr. J. H. Aveling, p. 339.)

Carbolized Sponge Tents.—Sponge suitable for tents may be generally got from the chemists in small pieces at a very small cost. These should be very thoroughly washed, then carefully dried, and cut into pieces two inches in length. The tents usually sold are far too long. The shape should be slightly fusiform. The sponge must now be tightly wrapped round with tape, spirally applied. An axis is required on which to roll the sponge whilst the tape is fastened round it, and nothing answers better for this purpose than a slender square sided instrument, called a "broach"; on a round wire, or other tool it constantly slips during the making. The tent must then be dipped into some melted cocoa-butter, or oil of theobroma, containing from 24 to 30 grains of white wax, and a drachm and a half of pure carbolic acid for each

ounce of oil. Tents containing carbolic acid as recommended do not emit any unpleasant odour when removed. The tape must not be removed when the sponge is dipped, the cellular structure of the latter being readily permeated by the oil through the tape. When the tent is quite solid the tape is to be cut and rolled off. The pure crystalline carbolic acid is the proper preparation to use. (Mr. R. Ellis, p. 333.)

Fibrous Tumours of the Uterus.—There are five ways in which nature may spontaneously cure a fibrous tumour of the uterus. First by interstitial absorption. This is very rare but still does undoubtedly occur ; it is generally preceded by softening. Dr. Matthews Duncan relates a case in which a tumour existed as large as a fœtal head at the end of pregnancy, but yet at the time of his writing no such tumour existed. Secondly, the tumour may become detached spontaneously, when it of course decomposes and becomes putrid. This only occurs to polypi. Thirdly, a fibrous tumour may become transformed into a calcareous mass, which ceases to grow, and may be partly or entirely discharged. This is a much rarer event than either of the preceding. A tumour which has undergone this transformation ceases to be productive of symptoms except such as may arise from its mechanical influence. It may be discharged *per vaginam*. Fourthly, the tumour may disintegrate and slough away. This process generally gives rise to symptoms of a grave kind, and in many instances the patient has actually sunk under the wasting effects of the discharge and the constitutional irritation. Lastly, the tumour may be eliminated by parturient efforts, the tumour being still in vital connection with the womb when the contractions begin. It is singular that this is more dangerous to the life of the patient than even the fourth or sloughing process. (Dr. A. H. M'Clintock, p. 357.)

Dr. Atlee, of Philadelphia, who has had much experience both in cases of ovarian tumour, and in fibrous tumours of the uterus, states that in his early practice he had excised large fibrous tumours, but the result had not encouraged him to repeat such operations unless the patient was in imminent danger. He had also become much less willing than formerly to incise these tumours by the vagina. He has had some cases in which the continued use of ergot and incision has been followed by the death and expulsion of large tumours, or their disintegrated remnants, and by the complete recovery of the patient; but the risk of uterine phlebitis and pyæmic fever has proved to be so great that he has become gradually less willing to interfere, unless driven to do so by hemorrhage or

some other condition endangering life. And he has become more hopeful of these tumours becoming indolent as age advances, and at length undergoing a process of atrophy or spontaneous cure. He has been in the habit of giving ten grain doses of muriate of ammonia three times a day, and thinks benefit has followed its use. Mr. S. Wells gives chloride of calcium with the object of promoting calcification of the vessels of these growths. (Mr. T. S. Wells, p. 363.)

Granular Inflammation of the Cervical Canal of the Uterus.—In this affection the cervical canal presents the appearance seen in granular ophthalmia, but the tint of mucous membrane is often more approaching a deep purple. As the os is generally more open than usual the dark granulations may be pretty easily seen by means of a speculum with direct sun-light. When seen in the cervical canal it is generally present also in the uterine cavity itself, and this may be verified by the use of the endoscope, by the aid of which the healthy may be distinguished from the inflamed patches of mucous membrane. The disease is curable enough by the application of caustics to the granular portions of the mucous membrane. The details of this treatment will be found described in the article at p. 344. (Dr. F. Churchill, p. 341.)

Induction of Premature Labour.—Undoubtedly the best way of inducing premature labour is that known as Cohen's. In this plan fluid is injected between the uterine walls and the membranes by means of a catheter. A great improvement on this is to use a tube with a single aperture at the end instead of with side apertures. The object of this is that the fluid may pass directly upwards to the fundus of the uterus. Experience shows that for sure action of injection, it is necessary that the fluid injected should approach as near as possible to the fundus of the uterus, this being the most sensitive to irritation. (Prof. J. Lazarewitch, p. 322.)

The following is the mode of application of the douche for the purpose of inducing premature labour. Place the patient in the usual obstetric position, with the hips drawn well over the edge of the bed; by passing a full sized Fergusson's speculum the os may then be brought into view and the nozzle of an ordinary syphon syringe inserted into it. A continuous stream of water must then be injected into the cavity of the uterus. Before using the syringe care must be taken to fill it completely with water, so as to exclude the admission of any air into the uterine sinuses. Tepid water answers very well, and it is unnecessary to use alternately hot and cold water. (Dr. T. Telford, p. 325.)

MENORRHAGIA.—*Plugging the Uterus.*—Supposing in a case of large heavy uterus with unhealthy granular mucous membrane, menorrhagia cannot be arrested either by plugging the vagina, or even injecting the uterus with a solution of iron, what is the proper treatment to adopt? Plug the uterine cavity itself by means of small pieces of cotton wool, to which threads of silk are attached for facility of withdrawal. These pieces of cotton wool must be dipped in a solution of sesquichloride of iron and introduced by means of a uterine sound, through a speculum. The plugs should be allowed to remain *in situ* for twenty-four or forty-eight hours, and on removal a number of fresh ones inserted. The probability is that the hemorrhage will be permanently checked, and the next menstrual period be normal. (Dr. Murray, p. 371.)

OVARIOTOMY.—*Extra versus Intra-Peritoneal Treatment of the Pedicle.*—The practice of tying the pedicle of an ovarian tumour, whether the ends of the ligature are left hanging out or are cut off short, is not so successful as that where the pedicle is brought outside the abdomen by means of a clamp. Sinus and abscess are the occasional results of the former plan, and not unfrequently a matting together of the intestines in the neighbourhood of the ligatures occurs, causing a liability to obstruction of the bowels. Patients who recover after the extra-peritoneal treatment of the pedicle, as a rule, soon regain and maintain perfect health. So do many of those who recover after the intra-peritoneal treatment; but some of them, sooner or later, suffer from chronic suppuration, hæmatocele, or fæcal fistula; or, perhaps, without any definite local ailment, are many months before they become strong and well. (Mr. T. S. Wells, p. 369.)

POLYPUS UTERI.—The author gives a report of forty-one cases of uterine polypus, all of which were removed by the annealed steel wire-rope ecraseur. He finds this mode preferable to that of excision, as there is no hemorrhage, and less tendency to purulent absorption. No death occurred in any of this series, and the removal was not in any case followed by hemorrhage. A point much insisted on is the necessity of guarding against the decomposition which is liable to occur in the clots of blood and uterine secretions. For this purpose a pint of warm water thrown up the vagina twice a day, with or without a little disinfectant, is very useful. There is one great advantage in the removal of polypi by "ecrasement," that by the crushing process a line of condensed tissue results, which doubtless forms a barrier to the ready absorption of unhealthy matter. (Dr. J. B. Hicks, p. 345.)

In a paper illustrative of his treatment in forty cases of polypus uteri, all of which ended successfully, the author states that having seized the polypus with a vulsellum forceps he always first attempts enucleation. For this purpose a slight scratch with the finger-nail has generally answered, and once begun it is easily completed, providing the tumour is within reach; sometimes a slight snip with the scissors may be required. Many tumours may be twisted off by a few turns of the vulsellum, but most require excision. For this purpose a pair of strong curved scissors, ten inches long, are required. Of the forty cases, nine were removed by ligature applied by means of the double canula. The material used was whipcord, and it was tightened morning and evening. A point of great importance in the use of the ligature is care not to include any portion of the uterus. The late Dr. Wm. Hunter lost more than one patient in this way, as he ascertained by examination after death. It is better not to include all the stalk of the tumour, as the part left perishes, and passes away in the discharges. (Dr. R. Dyce, p. 351.)

Puerperal Fever.—An interesting case of puerperal pyæmia is related by Dr. Snow Beck in which after death the uterus was found large and flabby, with the uterine sinuses so patent as to admit of fluid being injected into them. These sinuses contained pus, the source of which was a portion of putrifying placenta adherent to the uterus. The case well illustrates the cause of one of the most fatal forms of puerperal fever. It appears very questionable whether puerperal fever ever occurs with a firm contracted state of uterus, and the opinion is gaining ground, though it is not undisputed, that purulent infection of the general system in all endogenetic cases occurs through the pervious state of the uterine sinuses. When purulent infection has occurred, the uterine cavity must be cleansed out by some disinfecting fluid, as a solution of iodine or of permanganate of potash, and the sulphates given internally. Dr. Barnes states that he has repeatedly seen puerperal fever after perfect contraction of the uterus, but Dr. Snow Beck says that the cases given in support of this statement were not real cases of puerperal fever. (Dr. S. Beck, p. 328.)

Puerperal fever may be divided into *two great classes*, with a few minor kinds, namely, that produced by decomposition of a clot, or the secretions in the uterus, thence called endogenetic; and that derived from zymotic poisons, of which scarlatina and erysipelas are the most common. In my own practice I have found that about three-fourths of the puerperal fever I

see is somehow mixed up with scarlet fever. Of all disinfecting fluids for application to the uterine and vaginal cavities the permanganate of potash is the preferable one, because it shows when it has done its work. (Dr. J. B. Hicks, p. 330.)

The taint conveyed from post-mortem examinations is a fertile source of puerperal fever. This is well illustrated by the statistics of the Vienna lying-in hospital, where at one time, when the students passed directly from their dissections to the lying-in women, the mortality was 1 in every 10 deliveries. When this arrangement was changed the per-centage of deaths fell to 1 in 74. (Dr. H. Davis, p. 332.)

Puerperal Fever and Erysipelas.—A circumstance is related, well illustrating the connection between puerperal fever and erysipelas. Some years ago a lying-in ward had been established at King's College Hospital. The utmost care had been taken in its construction and management, but in spite of every precaution the mortality had been for the last year or two excessively high. A short time ago numerous cases of erysipelas appeared in the surgical wards, and immediately afterwards the two most recently confined women were attacked with a very adynamic form of puerperal fever, which proved fatal in both instances. There could be no doubt that the cause of the disease was the same as that which was producing erysipelas in the surgical wards. (Dr. Playfair, p. 329.)

Vaginal Examinations.—In vaginal examinations at hospitals, where many women are examined in succession, great care should be taken to secure cleanliness and prevent infection, by anointing the finger before each investigation with a compound of purified soft-soap, glycerine, and carbolic acid, in lieu of grease or oil, and the subsequent use of a solution of Condy's fluid. ℞. Glycerini, spir. vini rect. āā ℥ss., saponis mollis ℥j., acidi carbolici ℥ss. (Med. Times and Gazette, Jan. 11, 1868, p. 34.)

The Vectis in Labour.—The vectis must be considered as an artificial hand. It may be made of the greatest use in protracted and difficult labour, not so much by using it as a powerful tractor, for which it is not adapted, but by gentle and sustained traction during a pain, and by keeping the head from receding between the pains. It is so readily applied, and may be carried so easily in the pocket that it seems astonishing it is not more employed than it is. (Mr. G. D. R. McCarthy, p. 321.)

MISCELLANEA.

Action of Belladonna combined with Opium.—If a small quantity of atropine (the $\frac{1}{60}$th of a grain) is combined with morphia when subcutaneously injected, the faintness, nausea, and tendency to retching which is occasionally produced by morphia alone, is never seen. The combination of opium with belladonna, whether given by the skin or by the stomach, is an excellent one, as the belladonna intensifies the action of the opium and also does away with many of its unpleasant effects. (Dr. J. Harley, p. 392.)

Bichloride of Methylene.—The author has now administered the bichloride of methylene for the performance of nine capital operations. In one case the insensibility was sustained for an hour and seven minutes; in no case was the insensibility less than thirty-five minutes. In all there was the following order of phenomena: a first stage, of slight excitement; a second of slight rigidity; and a third, very prolonged, of relaxation and complete insensibility. If the inhalation is carried too far we have a fourth stage of prostration, which is immediately preliminary to death. The prolonged anæsthesia is the most striking feature of its action; but there is no occasion for anxiety from, nor for interference with, this sleep; it passes off quite naturally if time be allowed, and what is more, it passes off leaving no headache and no feeling of depression behind. Bichloride of methylene is at least as safe as chloroform, and it possesses certain advantages of its own which give it a better position amongst anæsthetics than chloroform. (Dr. B. W. Richardson, p. 398.)

Professor Nussbaum, of Munich, has been employing this anæsthetic, but does not consider it preferable to chloroform beyond being of a pleasanter odour. He found the effects produced in all the stages of its administration to be scarcely distinguishable from those produced by chloroform, except that complete return of consciousness occurred somewhat later. (Medical Times and Gazette, Jan. 25, 1868, p. 111.)

Disinfectants.—All the organic poisons so important in the causation of disease possess a more or less complex constitution, yet of very little stability, as evidenced by the fact of their decomposition at about 212°. Hence arises our hope of their direct destruction by oxidation. From this it follows that our principal disinfectants are oxidizing ones, which render the poison harmless by reducing it to its simplest form. Antiseptics are the second class of disinfectants, these place the matter in a state unfavourable for its destruction by oxidation or otherwise. (Dr. W. Procter, p. 18.)

Internal Antiseptics.—The sulphites are the most readily absorbed of our internal antiseptics, but carbolic acid is the most powerful. The great desideratum is a salt which shall combine the two. This desideratum the author has succeeded in obtaining in the production of the sulpho-carbolates. (Dr. A. E. Sansom, p. 10.)

Poisoning by Carbolic Acid.—Care must be taken in the external application of carbolic acid over large surfaces, as three cases of poisoning by it have occurred in the workhouse of Aston Union, near Birmingham. It was applied in a wholesale manner in mistake for sulphur lotion for the cure of itch. Two of the cases proved fatal. (Mr. E. S. Machin, Brit. Med. Journal, March 7, 1868, p. 220.)

Poisoning by Cyanide of Potassium.—The best antidote is a strong solution of sulphate of iron. As an accident from the cyanide is most likely to occur where photography is carried on, the remedy above-named is generally at hand. (Dr. Alfred S. Taylor, Guy's Hospital Reports, 1868, p. 239.)

Snake Poison.—Mr. Charles J. Smith, late Inspector-General of Hospitals, Madras Army, after making many experiments on dogs, and witnessing the effect of the antidote he recommends in a case of snake-bite in a man, considers that in the liquor ammoniæ diluted just sufficiently to enable the patient to swallow it, we possess an efficient remedy. A tourniquet ought to be placed on the limb bitten, artificial respiration kept up if necessary, and the liquor ammoniæ freely administered. It is curious that neither prussic acid, opium, nor arsenic are poisonous to snakes, whilst tobacco is so fatal to them that a pinch of snuff is sufficient to poison them. (British Medical Journal, Feb. 22, 1868, p. 164.)

Protoxide of Nitrogen.—This gas has been pretty well tried as an anæsthetic at St. Bartholomew's and also at University College Hospitals. It acted pretty well, but there is a general feeling that it is not comparable with chloroform in many important particulars. Useful where a single tooth has to be extracted, most persons would prefer taking chloroform had they to lose three. (Lancet, May 2, 1868, p. 563.)

Septicides.—The author states that he has succeeded in producing a series of salts which combine the diffusibility of the sulphites with the potential efficacy of carbolic acid. These salts are the sulpho-carbolates. He does not agree with Dr. Polli in his idea that the sulphites do not act upon the organized germs of disease, but upon the pabulum in which these germs are propagated. He believes they act directly as septicides. (Dr. A. E. Sansom, p. 6.)

Subcutaneous Injection of Morphia.—*Dangerous Results from.*—We cannot feel quite comfortable in the constant use of subcutaneous injections after reading a case which occurred to Mr. Braine, of London. The patient fell back to all appearance dead, and was only brought round with great difficulty. It could not well have been from air entering a vein, for the injection was made just over the insertion of the deltoid. Great care should be taken that the syringe when charged with the solution to be injected is free from air, and is clear from minute floating particles. (Medical Times and Gazette, Jan. 4, 1867, p. 8.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. 1.—THE EPIDEMICS OF TYPHUS AND CHOLERA IN LIVERPOOL.

By Robert Hamilton, Esq., Surgeon to the Southern Hospital, Liverpool.

[The question as to the fungoid origin of typhus and cholera is becoming the question of the day, and is gradually exciting amongst us an interest still very imperfectly proportionate to its immense importance. Modern investigation is fast leading us to the belief in the cryptogamic origin of many diseases; and though we may be staggered by the suggestion that such a disease as rheumatism has its cause in the sporules of a fungus, we are not so when it is attempted to prove that the germs of other fungi have to do with the development of cholera and fevers.]

That we are on the eve of arriving at the conclusion that the malarious fevers of hot climates, that dysentery, yellow fever, and cholera, are all caused by the action of different species of fungus or of infusoria, seems extremely probable. And if so, the time is not far distant when further investigation will prove that enteric and typhus fevers, as well as scarlet fever, measles, and small-pox, have a like origin.

The epidemic of pestilential intermittent fever at the Mauritius is perhaps the latest, as it has been, in proportion to the population, the most fatal of these scourges of mankind; and the facts already published concerning it connect malarious influences with its outburst, whilst its afterspread was aggravated by contagion, by contaminated drinking-water, and by exposure to the emanations from the dejecta of infected persons. And such, too, is the history of the yellow fever in its rise and progress, in whatever country it occurs; of epidemic dysentery in its home in India; and of cholera. Each is dependent upon its own peculiar fungus for the train of symptoms which ensue; and this, again, belongs to that genera or species which the animal and vegetable life of the country where it grows tends to produce.

The fever at the Mauritius has led to further discoveries. The investigations of Dr. Schmidt have proved the presence along the whole of the intestinal canal, of a patient who had died from the fever, of minute plants of a fungus, the counter-types of similar growths found by the aid of the microscope in the water of the Grand River. Can we resist the belief that the fever owed its existence to the presence of these growths?

The difficulties attendant upon all microscopical examinations of the stomach, intestines, and their contents, are so great, and the conclusions derived from the same so liable to be erroneous, when the search is for organic materials, that it requires the other links in the chain of evidence to be many and strong. We may at least aver that external facts do point very forcibly to the cryptogamic origin of all the most virulent of the infectious diseases, as yellow fever, cholera, and the marsh fevers. In these instances, it seems, as it were, that a close scrutiny and examination of the tissues and organs of the sufferer who has succumbed to one of them must lead to the discovery of the presence of organisms, vegetable or infusorial, which have given rise to his particular malady: and doubtless it will do so. The very fact of Dr. Schmidt having detected them in his fever patients, and Hallier in the evacuations of a cholera patient, will stimulate other microscopists to examine and test the truth of the discovery.

The blood in several types of fever, as well as in cholera, has hitherto failed to exhibit, under the most carefully repeated microscopical examination, the presence of these growths, or of anything abnormal. And this has proved a stumbling-block to many. Preoccupied with the belief that the majority of these diseases are blood diseases—that continued fever, for example, is the very type of a disease which consists in the poisoning of the blood by some deleterious agent from without, and failing to find its presence in what seemed its chief seat, they have abandoned, and even opposed, the doctrine of the fungoid origin of fever. It is remarkable that neither Hallier nor Schmidt found in the blood of their patients any of the spores which they yet discovered so abundant in their stomachs.

I must not weary the readers of the Lancet by detailing facts connected with the yellow fever in the West Indies and Africa, or with the rise and progress of the cholera, or with the various intermittent fevers and dysenteries of tropical climates. An examination of the past volumes of the Lancet alone, where particulars of a great number of these outbreaks are given, would suffice to show that the general condition of the country or district where these diseases prevail is the same in all. Rank vegetation, stagnant waters, a polluted atmosphere, or one damp and reeking with the products of decaying animal and vegetable

matter, contaminated water, with heaps of human ordure lying near it, are more or less characteristic of them all. Dr. Livingstone, in writing to a friend, stated that around every village on the Zambesi there was a very large collection of human ordure during the dry season, which was swept away into the rivers by the heavy rains; but he adds, "What must be the effect of this filth from hundreds of thousands of villages upon the stream? And though they do not take their drinking-water from it, but dig holes in the sand close by, yet we can understand how readily by percolation the water in the sand-holes has become impregnated with noxious matter. All the rivers on the West Coast of Africa—the Niger, Old Calabar, the Congo—are in the same way polluted."

It is very important to bear in mind that these streams, and likewise the sewers, drains, and cesspools of towns, which hold in suspense the germs of many a poison, may, and doubtless do, contain other ingredients which destroy the vitality of the germs, and so we account for their limited spread. Countless myriads of them are rendered innocuous, or their organisation destroyed, and new combinations formed out of their elements, by the action of other ingredients held in suspense in the same medium. This thought is a very suggestive one; and, if we but follow it up, we see at once how large and counteracting a cause to the spread of infectious diseases is ever at work for our preservation. Not all, or probably one-thousandth part, of the sporules given off by one seed-vessel fall to the ground, or there meet with favourable conditions for their fructification and development. The birds may devour them, frost or cold may destroy their vitality; the presence of a noxious gas or emanation in the air, or an atmosphere charged with deleterious influences, may neutralise their power of germinating; and, then, what becomes of them? They do not remain unchanged; but when the subtle essence of their life is gone, they break up at once by the action of surrounding matter, and their constituents enter into new combinations. But allow that they do commence an independent existence,—let the seed fall to the ground, and under the favouring conditions of heat and moisture burst its capsule,—even then it is but to meet with, in the majority of instances, new causes inimical to its existence; for, on the one hand, whilst it has overcome many obstacles, on the other it requires certain fresh conditions without which it cannot grow. To mention but one of these, as perhaps the most important: a congenial soil, one not charged with matters that would be destructive to it,—and we can understand how, though the seeds and spores and germs of vegetable life may be as the sands of the sea-shore in number, yet the germs and spores of animal and infusorial life are equally so; and the one is so often

inimical to the coexistence of the other that barrenness, death, dissolution, recombination into new forms, is the almost universal fate of both. If they are not always destructive to one another, there are yet other agencies to bring about their destruction; and, in the mineral kingdom, we have forces at work in the evolution of gases and acids, the effect of which upon microscopical animal and vegetable life, though but little understood at present, may be safely affirmed to be opposed, as a rule, to their further growth or development.

In these things we find an answer to those who say that, if infectious diseases had their origin in the sporules of a fungus, each disease would be much more wide-spread and universal than it is. Again, we have no reason to think that the fever or the cholera-breeding fungus is ever so abundant as that of most vegetable fungi; the very rarity of its appearance, its comparatively limited spread, and its disappearance after a time, are no arguments against its existence, but simply demonstrate its delicate organisation, its easy destruction, and the rarity of the combination of conditions necessary to produce it. The history of infusorial life is, in many of its aspects, similar to what has here been said of the vegetable world; and, as the two kingdoms approximate so closely in their most primitive forms, so are they similarly acted upon by external influences. It may be that some diseases take their rise in the entrance of the spores of certain species of the infusoriæ, and some others in that of the germs of certain genera of fungi. The cells of either, whilst floating in the air, must be inconceivably minute; but when introduced into the stomach by the food, or perhaps (as they most commonly are) by the saliva, they there find material congenial to their germination; or, on the other hand, destructive matters may be present. In the latter case, they are resolved into their ultimate elements, and, entering into new combinations, cease to be noxious, if they originally were so. In the former, a development probably of the most rudimentary form is produced, and this, though germinating within the stomach, may be as harmless as thousands of its species are, or as poisonous in its degree as its larger brother growing in the field or buzzing in the air. Speedily developed thus far, it now requires other conditions than are to be found in its present habitat, and it as speedily dies; but if possessed of noxious properties, it and its fellows who entered with it have, during their short but active existence, succeeded in poisoning the nutrition of the body. The exact mode in which they have done this remains a still unsolved problem.—*Lancet, Nov.* 23, 1867, *p.* 640.

2.—INTERMITTENT FEVER SUCCESSFULLY TREATED WITH STRYCHNIA.

By Dr. JOHN PEARSON NASH, Surgeon in H.M. Madras Army.

[Dr. Nash was induced to try strychnia in certain cases of ague, from the failure of quinine in effecting a cure. Mercara is the head-quarters of the district of Coorg, and is situate at an elevation of 4000 feet above the level of the sea. The climate is cool and pleasant though extremely unhealthy at certain seasons of the year.]

Case 1.—Mr. J. H——, a coffee-planter, residing on his estate, eight miles from Mercara, had been suffering from intermittent fever for three months, the paroxysms recurring every third day with great severity, and lasting about ten hours. After having taken large doses of quinine for several weeks without any benefit, he sought my advice, and presented the pale and emaciated appearance of a person who had suffered severely from ague. I prescribed the one-sixteenth of a grain of strychnia, to be taken four times a day, which immediately checked the paroxysms, and the patient was able to return to his estate after having been only five days under treatment, and wrote a fortnight afterwards to intimate that he still continued free from fever.

Cases 2 and 3.—Two daughters of the chaplain of the station had suffered from repeated attacks of intermittent fever, and having failed to derive any benefit from either quinine or the arsenical solution, were at last recommended a change of climate. After two months' absence, they returned to Mercara, and again contracted intermittent fever, the paroxysms of which commenced every other morning at six o'clock, and continued until noon. As the ages of the young ladies were 9 and 11 respectively, only one-thirtieth of a grain of strychnia was ordered to be taken four times a day in some strong infusion of quassia. In six days the younger girl was free from attacks of fever, and in eleven days the elder one had also recovered.

Case 4.—Captain P. J——, the proprietor of a coffee estate in a very malarious portion of the district, who is continually suffering from fever, and over which quinine has long since ceased to exert any influence, was ordered the one-twentieth of a grain of strychnia four times a day. On the eighth day of treatment the patient expressed himself as being perfectly well, and free from fever.

Case 5.—J. C——, a carpenter in the Mercara gaol, has suffered severely from repeated attacks of intermittent fever, for which he has taken an enormous quantity of quinine, and has also been treated with the arsenical solution, but without any benefit from either mode of treatment. He was therefore

ordered the one-twelfth of a grain of strychnia four times a day, under which treatment he rapidly recovered, and has continued in good health ever since.

I find from some tables prepared by my medical subordinates that in the Regimental Hospital, Gaol, and civil Dispensary at this station, thirty-seven cases of intermittent fever have been lately and successfully treated with strychnia, with an expenditure of a little less than one drachm of the alkaloid, and that the average period of recovery was on the eighth day of treatment; whereas in the thirty-seven preceding cases treated with sulphate of quinia, with an expenditure of fourteen ounces of this salt, recovery did not result until the twelfth day of treatment.—*Lancet, March* 14, 1868, *p.* 344.

3.—ON THE USE OF SEPTICIDAL AGENTS IN DISEASE.

By Dr. Arthur Ernest Sansom, Physician to the Royal Hospital for Diseases of the Chest, City-road.

[The use of sulphites in zymotic diseases was first advocated by Polli in 1857.]

It was found that animals could, without any apparent ill effects, swallow and absorb large doses of the sulphites. It was then observed that when the animals were killed they long resisted the putrefactive process—that whilst an animal killed under ordinary circumstances exhibited rapid and marked signs of putrefaction, an animal which had previously absorbed doses of the sulphites betrayed no sign of putrefaction whatever. Another series of experiments, and in this series three hundred dogs were the bases of the deductions, showed that the sulphites exerted a prophylactic and curative power when septic poisons were introduced into the economy.

Then, as regards the human subject. It was found that the stomach would tolerate large doses of the sulphites of soda or magnesia. They were tried in the various eruptive fevers, intermittents, typhus, typhoid, pyœmia, puerperal fever, dissection wounds, malarial infection, &c. The records of cases treated in this way show an extraordinary amount of success. Dr. Polli states—"I published my first memoir on this subject in 1861, and since that time one hundred and fifty-eight papers have been published in answer to my call; and, with the exception of five or six containing some criticisms on my labours, all the remainder confirm, in the strongest terms, by many hundreds of detailed observations, the value of these remedies."

I can endorse this statement from my own personal experience. I have employed the sulphite of soda in the eruptive fevers, in malignant sore throat, in diphtheria, as well as in

cholera and choleraic diarrhœa. In measles I have used it in a number of cases, always with apparent success. In scarlatina I have employed it, both internally administered and as a gargle; its efficacy in anginoid symptoms has been marked. In diphtheria I have employed it in like manner in three cases. These were all extremely severe; all but one recovered, the fatal case being a child of 2 years, to whom none of the sulphite was for twenty-four hours, by a mistake in the nursing, administered. In a case of small-pox the effect seemed to be wonderful. The face was completely covered with pustules, the drug not being administered till the pustular stage had arrived. There was extreme prostration, the pulse was, on November 7th, 1866, 104; the sulphite of soda, in scruple doses, was assiduously administered every four hours, and in two days the signs of danger passed, the pulse falling to 76 on November 9th. I notice that Dr. Nichol, in the *Nashville Journal of Medicine and Surgery* for August, 1866, testifies to the efficacy of the sulphites in small-pox. For cases illustrating the value of the sulphites in cholera and choleraic diarrhœa, I can refer not only to my own volume, but to the narrative of cases in my first communication to your columns. The cases there mentioned were all treated by the sulphite of soda method, and all recovered.

If it be granted that the sulphites are useful in the infecting diseases, what is their *modus operandi?* In my answer to this question I am forced to differ from Professor Polli, who says, "these salts do not act as poisons towards the several morbific ferments. . . . They do not kill the catalytic germs of the organic poisons; but they re-act on the material components of our own organism, rendering it, by their presence, incapable of being acted on by these catalytic germs." The mode in which Polli supposes they affect the "fermentable stuff" of the organism is a process of deoxidation. I have before said that sulphurous acid and the sulphites are not mere deoxidizers, but that have a special influence in arresting the vitality of low organisms. Moreover, there are many arguments against Polli's hypothesis that the sulphites act not on the ferment but on the pabulum, wherein the ferment undergoes development. In the first place what does physiological chemistry tell us of concerning the action of the sulphites in the economy? Dr. de Ricci says, "Animals after having been saturated with sulphites were killed, and every portion of their body, solids and fluids, gave, on examination, unquestionable proof of the presence of the sulphites employed. These experiments seemed to prove that when sulphites, whether alkaline or earthy matters or not, are introduced into the stomach of a living animal, they may be absorbed and circulated *as such* through the organism." Fur-

thermore, "Dr. Polli found in men as in dogs, that they are not ordinarily decomposed in the stomach but pass unchanged into the blood, or, at least, appear in a brief period unchanged in the urine, and only after a while as sulphates." From these facts alone it may be concluded that the deoxidizing power of the sulphites must be limited indeed, for that power could be strictly determined by their transformation into sulphates. Furthermore, if the efficacy of the sulphites depended on their deoxidizing power over the fluids of the organism, consider what an enormous quantity would be required to effect such an object! What does analogy teach us? A trace of sulphurous acid or an alkaline sulphite will arrest the fermentation of a large quantity of saccharine material. The same quantity would not suffice to deoxidize the smallest fraction of this material; therefore, its efficacy must lie in some other quality than its power of deoxidation. Analogy tells us that it has a peculiar power, as I before stated, of arresting vitality; this power is catalytic not chemical, and finds its chief exemplum in carbolic acid. There is nothing antagonistic to reason in the conclusion that substances which are perfectly harmless in the case of a high organism should be fatal to a lower organism. Common salt, among a multitude of others, presents an instance in point. I think, therefore, we cannot fail to come to the conclusion that the sulphites are readily absorbed by the higher organisms—that they circulate in the economy for the most part unchanged—that they exert a peculiar power, not upon the pabulum of the body but upon organized particles which may exist within it; and that in virtue of this septicidal power they manifest those prophylactic and curative effects which both experiment and experience have proved them to possess. We now arrive at

Proposition D.—That the germs of cholera are capable of destruction within the living body.

It may seem that this proposition admits of an easy discussion, for, according to our former conclusions, the argument might stand thus: Diseases which depend on morbid germs are to be successfully treated by sulphites or such-like antiseptics. Cholera is a disease which depends on morbid germs; therefore cholera is to be successfully treated by sulphites, &c. But this conclusion is not at this time justifiable; it can only be said that an *á priori* case has been made out.

Many circumstances may militate against the success of a treatment, though its principle may have been satisfactorily enunciated. The virus of cholera may induce such rapid effects that remedial agents of sufficient power may not be capable of withstanding it in time; or the poison may have a long period of latency, which no symptom may betray until its hold on the

body has been fatally strong; or it may have a more intense power of proliferation and diffusion throughout the body than the poisons of those zymotic diseases heretofore mentioned. Or, perhaps, the sulphites which have been shown to be efficacious in other diseases, may not be powerful enough, or administrable in sufficient quantities, to kill the germs of cholera. It may be, therefore, that though antiseptics can destroy the virus of cholera outside the organism, internally administered they may be powerless. It was greatly thus in the case of cattle-plague: a mass of evidence proved the antiseptic power of carbolic acid; but the internal administration, though apparently in some cases successful, was not attended with an advantage proportionate to the value of its external employment.

Professor Polli did not claim for the sulphites the probability of a success in cholera. "Dr. Polli does not venture to assert that they will prove available in cholera or the plague; but he thinks there is reasonable ground for concluding that they will be highly useful in prophylaxis, in destroying the fermentable material on which the malignancy and fatality of these pestilences chiefly depend, and thus, render an inevitable attack mild. This opinion, however, is founded on the hypothesis that the sulphites act, not upon the ferments, but upon the "fermentable material"—a hypothesis which I have considered unsound. The antiseptics act directly on the virus. It behoves us, as we have done in the case of other zymotic diseases, to take the evidence afforded by actual cases.

I have related in my book on cholera, cases in which the treatment by sulphite of soda was successful. All those cases related in my former communication to these columns on the Cholera Poison, were treated by the sulphites, and they seemed to show unmistakably the advantage of the mode of treatment.

Carbolic acid, internally administered, has been testified, by many observers, to have been successful in cholera during the recent epidemic. I find that so far back as December 17th, 1862, Dr. Godfrey advocated the administration of carbolic acid in cases of gastric irritability, especially when produced by miasmata or sewerage exhalations; he anticipated much good from it in the treatment of cholera.

The difficulty which has been urged against the antiseptic treatment of cholera, especially by those reviewers who have noticed my book, has been that the agents administered, could not be sufficiently rapid in their action to neutralize the cholera-poison. We know, however, that the sulphites are like other salts of alkalis and alkaloids very rapidly absorbed and circulated throughout the organism—a few minutes sufficing for their diffusion throughout the economy. The argument, there-

fore, against the sulphites would lie not against the *rapidity*, but the *sufficiency* of their action.

On the other hand, carbolic acid is, I think, proved to be the most powerfully septicidal agent known. The difficulty in this case is its deficient absorbability. It is undoubtedly an irritant, as may be at once proved by applying a little of it to the skin—the stomach will not tolerate doses of more than two to five minims, and of this amount it is probable that only a small proportion is absorbed into the blood.

The great desideratum, in my opinion, has been an agent which should supply the qualities of the rapid diffusibility of the sulphites, with the potential efficacy of carbolic acid. This desideratum I believe now to have been supplied. Some months ago I saw, at the house of my friend Mr. Crookes, a specimen of a compound salt of sulphuric and carbolic acids with potass. I determined to pursue the method of investigation which thus opened out to me, and I have now succeeded in producing the compound salts of potass, soda, ammonia, and magnesia. I have found them to possess every quality which could be required of them, and a record of my practical experience with them I only defer to a future time.—*Medical Press and Circular, Jan.* 22, 1868, *p.* 61.

4.—ZYMOSIS WITH SPECIAL REFERENCE TO CHOLERA.

By Dr. Sansom.

[The following is an abstract of a paper read before the Medical Society of London.]

The author commenced by giving an outline of the theory of zymosis. In tracing the origin of infecting particles, we may, he said, divide them into two classes—First, those arising from the animal world, such as variola, vaccine, pyæmia; and secondly, those arising from the vegetable world, as favus, thrush, and, if we are to believe a large mass of scientific evidence, diphtheria, ague, &c. But whether animal or vegetable, it cannot be determined with accuracy whether the materies morbi is, at the period of infection, one or the other, It is best, under such circumstances, to call it "germinal matter." Dr. Sansom then related a series of cases which had occurred in his practice, all of which were united by close relations of time, place, and circumstances, and in one of which the "oidium albicans" was discovered as a prime factor in the disease. The author then discussed the operation of disinfectants. He divided them into three classes—First, those which alter the chemical constitution of the materies morbi, such as chlorine and iodine; secondly, those

which act partly chemically and partly vitally, such as the sulphites; and thirdly, those which act only on organised material, arresting vitality, such as carbolic acid. The treatment of zymotic disease by the internal administration of the sulphites was then considered, and forty-one cases were brought forward in which they had been employed, and in which one death only occurred. The facts seemed to be that the sulphites are the most easily absorbed of our internal antiseptics, but that carbolic acid is the most powerful. The author concluded by saying that the great desideratum was a salt which combine the two. This desideratum Dr. Sansom had succeeded in fulfilling, and specimens of compound salts, the sulpho-carbolates, were exhibited to the Society.—*Medical Times and Gazette, Feb.* 22, 1868, *p.* 216.

5.—CLINICAL EXPERIENCES WITH DR. RICHARDSON'S "STYPTIC COLLOID."

By Dr. JOHN LOWE, Surgeon to the West Norfolk and Lynn Hospital.

In ordinary operations performed on healthy tissues, Nature is, as Hippocrates said, "sufficient of herself to the cure of every evil." She pours out a protective fluid for the closure of the wound, and prevents those untowards results which follow the production and absorption of noxious products. But in less healthy structures, or where closure of the wound is impracticable, these results are certain to follow in a greater or less degree, and it is in such cases that we need assistance in controlling the tendency to decomposition in the fluids, and thus preventing the risks from pyæmia, while at the same time fostering and protecting the delicate cell growth on which we depend for the completion of healing.

Owing to the antiseptic principles (gallic acid and benzoin) contained in the styptic colloid, we find this indication completely fulfilled. The healthy character which the pus assumes after its use in necrosis operations, contrasts wonderfully with the usual sanious and offensive secretions when other dressings are used.

Case 3, which is narrated below, was one of the most striking I have ever seen, both as regards the speedy production of healthy pus and the rapid cure effected. Many conservative operations depend mainly for their success on the degree of immobility which it is possible to maintain. Frequency of dressing is in these cases the *bête noire* which we most dread. With the most delicate handling, and with the Surgeon's constant personal care, how difficult it is to avoid doing mischief!

Without them how often failure results! But if these be no longer necessary, except in a very minor degree, the gain is great indeed. The case of amputation of the foot shows well how little of this kind of supervision is required. Those who have had to undergo severe operations would soon tell us the worth of a process which would diminish the number of dressings to two or three.

But there are other cases in which we find equal advantage from the use of the "colloid." Burns, which are both troublesome, offensive, and painful in the dressing, are deprived of half their terrors. When profuse suppuration sets in, the "colloid," if painted freely over the surface, at once removes the fœtor, protects the granulations, and the wound speedily heals. No doubt the first dressing is painful, but not so the others. With moderately sized wounds, the first dressing is also the last. Nothing further is required but daily painting with the "colloid." In cancer the comfort obtained is very great. I have given a case in which the colloid was used with most perfect success, and strangely enough, with a marvellous diminution in the amount of pain.

Taking into consideration all the advantages which we obtain from the use of this preparation, and without departing from a sober estimate of the value of the facts observed, I cannot but conclude that we of the Profession, and above all the public, are under great obligations to Dr. Richardson for his simple but most elegant and useful discovery.

[Dr. Lowe then gives a case of amputation of the foot, by Pirogoff's operation.]

On May 13, the patient being put under the influence of chloroform, I proceeded to perform Pirogoff's operation, with the modification which I had previously adopted—viz., the preservation of the periosteum of the tuberosity of the calcaneum, the bony structure being enucleated by means of the gouge. The ends of the tibia and fibula were found to be sound, but a large portion of the anterior flap was gelatinous and very unhealthy. Owing to the contraction of the Achilles tendon, it was found needful to divide it. No vessel required ligaturing; the flaps were brought together by silver sutures. About an hour afterwards the wound was closed up by a free application of styptic colloid and lint, in the manner described by Dr. Richardson. Very little blood had been lost during the operation, and, with the exception of the anterior flap, $\frac{3}{4}$ concerning which grave doubts existed, the case promised a successful termination.

2nd day. Doing well; not much pain.

4th day. As before. No discharge.

5th day. Slight discharge; no smell whatever. Colloid applied freely over the wound.

8th day. The colloid has been applied daily. There has been pretty free discharge, but no odour. On dressing the stump for the first time, it was found that a large piece of the anterior flap had, as was feared, sloughed. The slough was not, however, detached. The sutures were removed; a couple of pieces of strapping applied to support the heel; colloid poured freely into the wound; lint and a light bandage applied.

16th day. There has been rather free discharge, but, thanks to the colloid, which has been applied daily over the bandage, no unpleasant odour. The stump was again dressed to-day, when the slough came away, leaving a healthy rapidly granulating surface. The end of the tibia was quite exposed, but showed no signs either of repair or decay. The heel was brought forward by strapping, and colloid applied as before.

On the third dressing the wound had nearly filled up by granulation; the tibia was no longer exposed; a thin shell of bone had separated from the outer margin of the fibula. There is now but little discharge.

June 19. Up to this time the limb has been kept perfectly quiet. The dressing has been applied daily without moving the stump. The wound is as nearly as possible healed, and there is only a trace of discharge. General health much improved.

July 10th. The stump has daily become firmer. Only one small spot about the size of a sixpence remains uncicatrised, owing to the traction of the reunited tendo Achillis, which had drawn the heel backwards.

20th. By the application of a splint to the back of the leg, this has been counteracted, and the sore is now all but healed. The stump is a well-formed one, with the heel nicely in the centre, and bids fair to render the limb a very serviceable one.

It is impossible to overestimate the value of such a dressing as the "styptic colloid" in a case of this kind. The perfect immunity from fœtor, the quiet state of the limb which is allowed, and the diminished risk of purulent absorption, together with the healthy granulation which is permitted to progress unretarded by the presence of a septic poison, are facts which create a new era in Surgery.

The perfect state of rest which was obtained in this case seems to have been the main element in its success. Three dressings only were required in the first three weeks—that is, the limb was only moved so many times. Judging from what I have seen of the action of the colloid, I am disposed to think that erysipelas will prove of much less frequent occurrence under its use, if, indeed, it be not altogether abolished in Surgical practice.

Case 2.—*Cancer of Mamma—Beneficial action of "Colloid."* —[Mrs. F., had first noticed a small hard tumour near the right nipple four years ago. She was advised to have it removed, but she refused her consent.]

The tumour continued to enlarge, and in July last ulceration commenced. The sore was five inches in length, extending from the nipple across the anterior border of the axilla. The remains of the breast closely adherent to the muscle. The axilla was filled with a mass of enlarged glands. The wound was sloughy, and intensely offensive. Continued lancinating pains deprived the patient of her sleep and appetite. She had lost flesh rapidly in consequence, and her face bore the appearance of cancerous cachexy in a high degree.

She was at this time quite willing to undergo the operation, and begged me to remove the breast. This, I told her was impossible, as the disease had advanced too far. I could hold out to her no hope save of mitigating her pain and making her last days more easy. To remove the horrible effluvium which made the poor creature a nuisance to herself and family, I painted the wound over with colloid, and gave her a bottle of it to take home with her. I also gave her a morphia pill to be taken at night. She came again the following week, when I was surprised to find that the wound had become quite clean, and was healthily granulating, every trace of smell having of course disappeared. But more than this, the pain had also gone; she had enjoyed comfortable nights, her appetite had improved, and, with hope in her countenance, she looked years younger than she had done the week before. She felt certain she would get well, but on this head I could give her but little encouragement, so extensive was the amount of disease. However, she continued to improve; the wound, which was constantly painted with the styptic, gradually filled up. No pain was experienced, except when she ran short of the dressing. The enlarged mass of axillary glands diminished *pari passu* with the wound, so that now they can scarcely be felt. The wound itself is not larger than a fourpenny piece (July 15), and would, I think, have been quite healed ere now but for the difficulty I have in inducing her to rest the arm, which, owing to her having a large family of small children, she has not been able to do.

Case 3.—*Caries of Tibia.*—W. A., aged 14, a delicate, strumous boy, residing at Snettisham, was admitted into the West Norfolk and Lynn Hospital with disease of the ankle-joint March 13, 1867. Several sinuses communicated with the joint, but the probe did not come in contact with any diseased bone. The limb was put up in a gutta-percha splint. Patient placed on a generous diet.

April 22. The sinuses continue to discharge, and now denuded bone is felt at the extremity of the tibia. Patient having been put under chloroform, an incision was made over this point. The articular end and about three inches of the shaft of the tibia were found to be soft and carious. By means of the gouge and pliers, the whole thickness of the bone was removed over this extent, care being taken to avoid the joint. The wound was filled with colloid, and closed by a piece of lint soaked in the same, the limb being placed again on a splint.

On the fourth day, when the limb was dressed, the wound was granulating very freely, and was filled with thick creamy pus instead of the usual sanious discharge one meets with in similar cases. The same dressing was continued, the wound rapidly closed, and on

May 13 he was made out-patient, only a small sinus, caused by a fragment of undetached bone, remaining. The splint was still retained, and patient was ordered to come up in a few weeks to have the fragment of bone removed. General health greatly improved.

The styptic has been largely used in our hospital in a variety of cases, and invariably with the best results. I have as yet never seen it fail to answer the ends for which it was designed. In old chronic ulcers, foul-smelling and filthy to a degree, the odour is at once destroyed, and healthy granulation established. Most markedly was this the result in a case of my colleague's. The patient, a young sailor, who had been long at sea, was admitted with a number of very large syphilitic ulcers on both legs, the stench from which made the ward unbearable. They were freely painted over with the "colloid," and in a very short space of time the odour was all but gone. They healed most rapidly.

In cancer of the penis, too, it is most serviceable in removing the odour and in mitigating pain. Nothing could be more striking than its action in this respect in the case of cancer of the breast which I have reported above.

In conclusion I may state that erysipelas is, with us, to all appearance, a thing of the past. Some time ago, owing to imperfect sanitary arrangements, almost every surgical case in the hospital was attacked with erysipelas, and several died of pyæmia. Some improvement was effected in the hygienic condition of the hospital, but it is still imperfect. Latterly, however, since using the tincture of iodine to the surface of the wound and styptic colloid externally, there has not been a single case of either erysipelas or pyæmia, and the cases generally remain a very much shorter time on the books. There is no doubt that the colloid is an expensive application, and that it causes a serious expense to the hospital; but I consider that the

gain from the rapid cure of the cases under treatment largely overbalances the original outlay; and not only is the expense diminished by the shorter duration of the cases, but, owing to the absence of septicæmia, there is a greatly reduced need of stimulants and extras.—*Medical Times and Gazette, Jan.* 25, 1868, *p.* 88.

6.—ALCOHOLIC RHEUMATISM FROM DAILY IMBIBITION OF FERMENTED ALCOHOLIC LIQUORS; AND ITS REMEDY.

By John Higginbottom, Esq., F.R.S., Nottingham.

About thirty years since, I first noticed that a form of (so-called) rheumatism was cured by abstaining for some time from the use of fermented alcoholic fluids. I said at that time, that the complaint should not be called rheumatism, but alcoholism, as *alcohol* produced the disease, and abstinence from *alcohol* was the remedy. Rheumatic gout and "the poor man's gout" may perhaps, for the most part, be that form of rheumatism; and gout, in some cases, may be superadded. I find Mr. N., a clergyman, has also noticed the fact in a work *(His Clerical Experience*, by the Rev. Thomas Rooke, M.A.) lately published. He says, referring to the effects of alcohol, "I may instance three men in our society—men advanced in life, who while drunkards were, as they express it, '*eat up with rheumatism*', now, that they are tried abstainers, are entirely free from it, and look fresher and younger by several years."

This form of rheumatism is produced by fermented alcoholic beverages, and has not been distinguished from rheumatism or gout. It appears usually about the middle stage of life, after the person has for some years daily imbibed fermented alcoholic liquors, such as beer, ale, porter, cyder, &c., even in moderate quantities. The disease advances insidiously, and the accumulating effect of the beverages produces great changes in the person, both physical and mental. There is an expression of stupidity or dulness in the countenance, a weakness of the of the intellectual powers, a stiffness and sluggishness of the body; the limbs lose their suppleness, causing a slight clumsiness of gait, followed by hobbling and ultimate lameness, which increases with age, and becomes permanent, if the drink be continued. This state probably arises from a change of structure in the synovial membrane, sheaves of the tendons, and the joints, when the complaint has the appearance of chronic rheumatism. This complaint does not appear to affect the general health.

Ordinary rheumatism arises from a far different source, and is caused by exposure to cold and wet, cold air, and by the variableness of temperature, insufficient clothing and food;

attacks mostly the young and strong ; is uncertain in the times of its attack, and characterised by quickly changing its seat from one part to another ; affects most of the limbs, the joints and the course of the muscles being successively affected ; and is attended by fever of an inflammatory type. Chronic rheumatism is a frequent result of the acute.

The cause of alcoholic rheumatism in this locality, I believe, is chiefly the effects of malt liquors ; but a writer observes that cyder-drinkers have a full share of the complaint. He says : "There must be something deleterious in cyder, as it is the precursor of rheumatism in the labouring man. Witness the number suffering from the effects of it, hobbling about at forty years of age ; the cyder countries are full of them." I have no doubt the complaint is alcoholic rheumatism, produced by the fermented alcoholic beverage from the apple as from malt, just as like results attend the daily use of malt liquor beverages.

Alcoholic rheumatism may be modified or prevented in a great measure, in those persons who do take fermented alcoholic drinks, by living on proper nutritive food and taking much exercise in the open air, which quickly throws off the alcoholic poison from the system, and prevents the evil. On the contrary, if the persons lead a sedentary indolent life, and indulge in taking abundance of food and fermented alcoholic drinks, they will probably inflict a double evil upon themselves—obesity, and alcoholic rheumatism or gout. If they are of a gouty diathesis, this will tend to increase their lameness and diminish their locomotive powers, as we see in landlords of inn, gentlemen's coachmen and butlers, and others of the same class. A farmer, who rides on his nag to overlook his labourers, and spends the afternoon and evening in his corner chair with his pipe and his ale, is a subject for corpulency and alcoholic rheumatism or gout, which may be, indeed, the precursor of paralysis, apoplexy, disease of the liver or kidneys, and also of calculi of the kidneys or bladder. The labouring men, with daily out-door exercise, although they may take their regular allowance of ale, and probably spend evenings at the alehouse, will probably have their share of alcoholic rheumatism, although free from obesity. Their drinking habits, hereafter continued, will bring on other diseases, and premature old age and death. Such men are frequently old at sixty years of age.

A remedy for alcoholic rheumatism is absolutely called for, as so large a class of men are suffering from it. I believe most cases may be treated successfully by abandoning the use of fermented alcoholic beverages altogether. I have a firm conviction that abstaining will cure most cases ; and, even where structural changes have taken place, the *pain* may be relieved, if not removed.—*British Medical Journal, Jan.* 11, 1868, *p.* 26.

7.—ON DISINFECTANTS.

By Dr. William Procter, F.C.S.

Dead organic matter passes rapidly into a state of change or decomposition, the nature of the resulting products being dependent upon numerous collateral circumstances. Amongst these changes putrefaction is specially distinguished by the fœtid character of the matter evolved. The more complex the composition of the bodies, the more unstable is their equilibrium, the more readily do their constituents enter into a state of change, and the more offensive are the emanations. Even the final products of putrefaction (especially when air is excluded or imperfectly supplied) are of this character, comprising, as they do, sulphuretted hydrogen, sulphide of ammonium, phosphides, ammoniacal, and other like compounds. It would not seem that the ultimate products of putrefactive decomposition directly generate zymotic disease, although they may produce their individual special effects, but that indirectly they promote its invasion by the exclusion of pure air, by forming a nidus for the nourishment of morbific matter, and, by depressing the powers of the system, render the body more predisposed and less resisting, and cause a more rapid spread of specific disease. It is rather to some intermediate product of the putrefactive process, and especially to the organic effluvia still in a state of change, that the power of setting up zymosis is to be referred. But the resulting gases may themselves either destroy life rapidly by acute poisoning, with symptoms well recognised, or produce a certain class of endemic diseases not transmissible under ordinary circumstances. Dr. H. Barker has submitted this matter to the test of experiment by conducting the air of a cesspool, containing carbonic acid, sulphuretted hydrogen, and sulphide of ammonium, into a box in which animals were confined; the symptoms which resulted resembled the milder forms of continued fevers common to the dirty and ill-ventilated homes of the lower classes of the community, and Dr. Barker attributes the results not to the organic but to the gaseous matter. The effects of these gases were then tried separately on confined animals, and he found that of sulphuretted hydrogen 0·5 per 1000 may produce serious symptoms, and 4·2 per 1000 was rapidly fatal, but not with the symptoms or pathological conditions of cesspool fever, while on the other hand, a volatile alkaline body, such as sulphide of ammonium, persistently administered, produced both the symptoms and pathology of fever. Dr. Dundas Thompson was one of the first to recognise the importance of organic matter as a constituent of the air of towns, and to enunciate the proposition that the gases evolved during putrefaction are not the main sources

of danger. The existence of a large amount of foreign matter in the atmosphere has been satisfactorily demonstrated. If pure distilled water in an open vessel is exposed to the air, the loss from evaporation being made up by the addition of fresh liquid, on the careful microscopic examination of the sediment which is formed, monads, amœbæ, and other of the lowest forms of life, will be detected. Pasteur placed soluble gun-cotton in a glass tube, and by means of an aspirator caused a current of air to pass through it for several hours. The cotton being dissolved, and the residue examined, was found invariably to contain organic growths, and he proved that the air of inhabited places contains a greater relative number of germs than the air of uninhabited regions. Over the mouth of an impure cesspool a glass globe was placed filled with ice ; on its surface the aqueous vapour, with its soluble suspended matter, was condensed. This liquid was turbid, had an offensive odour and alkaline reaction, and contained small flakes of matter which under the microscope had the appearance of organic *débris*, and with it there were vibriones, monads, and other low forms of life, with confervoid and fungoid filaments. Somewhat similar results are obtained from the vapour condensed by placing a bell-jar over putrid meat. Experiment has shown that in such an impure atmosphere milk is rapidly changed, and meat speedily putrifies. It is eminently a suggestive fact that these germs are not everywhere present in all forms and equal numbers. They exist numerously in the lower strata of the air and in densely populated towns, becoming fewer as we rise higher, and at such elevations as the summit of the Alps they are almost absent.

These conditions, then, are general ; but in certain localities special germs are detectible. M. Chalvet has collected putrescent organic matter from the walls of hospital wards ; and when watery vapour near a suppurating surface was collected, it was found to be charged with irregular corpuscles resembling dried pus. Eiselt, of Prague, found small cells like pus cells, in the air of a ward in which epidemic ophthalmia was raging.

These facts affords just reasons for concluding that the air is a medium through which diseases may be transmitted either by specific germs or by the presence of organic matter in a state of change which may set up certain so-called endemic diseases. In every instance of communicated morbid condition, there is material cause, however subtle its nature and mode of transmission, which must be concerned in its propagation. The power to communicate certain diseases is a material substance, in all probability made up of solid non-volatile particles, certainly not gaseous. Probably, then, it is a germinal cell, of which

vaccine lymph may be taken as the type, of peculiar organisation, capable of being transmitted from one locality to another, of preserving its vitality, for a time at least, outside the organism, and within of reproducing itself.

We can imagine two ways in which poisons may act on the system.

1. When virus is introduced into the system, it multiplies rapidly, and seems to have the power of transforming healthy matter into matter of its own nature, as small-pox. 2. By catalysis, a minute quantity of virus being sufficient to set up the septic changes in the blood to which it has gained access. Carefully conducted experiments by numerous observers have shown that the composition of the blood during disease undergoes alterations and variations. Purkinje says that the blood in cholera contains pure urea and an extractive substance by which the urea is rapidly converted into carbonate of ammonia. Diseases closely resembling those which occur naturally may be set up artificially, by introducing into the circulating fluid substances capable of acting catalytically. Abscesses have been produced by injecting pus into the veins of dogs, septic affections by the injection of putrid purulent matter into the veins, and diseases with all the characters of typhoid fever, by the introduction of putrid blood into the circulation. It must be admitted that countless germs of vegetables, infusoria, &c., exist in the air, and grow and multiply whenever they find an appropriate nidus. This condition of the atmosphere has been called by Dr. Sanderson septic, and derives importance from the possibility of being concerned in the production of zymotic disease. That these low forms of life may seriously affect the blood of the higher order of animals, is clearly proved by the recent researches of Davaine, who has furnished the first well-established example of a disease of the blood due to the presence of inferior beings. A low form of bacteria was discovered in the blood of sheep suffering from splenic apoplexy, and considered by the observer to be the cause of that disease. Dr. Salisbury, of Ohio, states that the prevalence of measles in the Federal army arose from fungi (a Penicillum). He was led to this examination by observing that a large number of men rose one morning with symptoms of measles, after sleeping on straw which was mouldy, and had a peculiar odour, and that, by inoculation with this fungus, he set up in many persons, in from twenty-four to ninety-six hours, a disease closely resembling measles. It is nevertheless proper to state that these experiments have been repeated by Dr. Woodward, of the United States Army, and that he does not confirm them.

Strong evidence has lately been brought forward to show that

fungoid vegetations, if not the cause, are intimately associated with malarial fever (Medical Times and Gazette, November 9, 1867.) Although it is true, as remarked by Dr. Snow in speaking of his opinion that cholera is disseminated by an especial cell, "that it is no objection to this view that the structure of the cholera poison cannot be recognised by the microscope, for the matter of small-pox and chancre can only be recognised by their effects, and not by their physical properties," yet special poisons have in many cases been isolated. Dr. Richardson has separated the poison of pyæmia; it may, he says, be evaporated to the form of syrup or extract. It produces when dried a substance closely resembling the snake poison. It admits of being pulverised, and when it is introduced into the wound of a healthy animal produces precisely the same symptoms as those of the patient from which the poison was taken. Panum's experiments on putrid infection are of high interest, but he will not attempt to decide whether the putrid poison acts "directly on the nervous system, or as a ferment to the blood." All the phenomena of disease would indicate that blood-poisoning is the result, and, as Dr. Richardson says, "that each particle of any one of these poisons, brought into contact either with the blood of the living animal or with certain secretions of the living animal, possess the property of turning the albuminous part of that same blood or secretion into substance like itself." Dr. Halford made some experiments on the poison of the cobra. He states that when a person is actually bitten by that animal, molecules of living germinal matter are thrown into the blood, which speedily grow into cells and as rapidly multiply, so that in a few hours millions upon millions are produced. Do their numbers render the blood unfit to support life, or, with some analogy to a process of fermentation, do they impoverish the blood by growing at the expense of some element of it, and at the same time excrete a something, a poison to which the symptoms of the disease may be directly due?

If, then, it be true that the presence of organic matter or the products of its decomposition are important elements in the causation of disease, the question arises—Is it within our power to remove those causes or to diminish or to prevent their injurious effects? The organic contaminations found in the atmosphere possess, for the most part, a more or less complex constitution analogous to that of organic matter in general, and, being of an oxidisable nature, are therefore extremely disposed to enter into combination with active oxygen, and form with that substance new arrangements with their elements, of a permanent character. And, again, from the little stability of composition of contagious poisons, evinced by the fact of their decomposition at about 212°, it appears that their constituents are weakly

held together. Here, then, one method is opened to us by which their action may be limited, by rapidly hastening their decay by a process of oxidation ; or, on the other hand, some substance may be employed which prevents the compound from changing its original composition. In this manner two classes of disinfectants are obtained :—1. Oxidising disinfectants, which hasten the decomposition of the compound, and at the same time render the resulting products harmless by reducing them to their simplest forms. 2. Antiseptics or colytics, which place the matter in as tate unfavourable for its destruction by oxidation or otherwise. To the preceding classes another division may be practically added, to which the term fixative has been applied—such as Burnett's fluid, &c., which entering into combination with the offensive volatile products, prevent the pollution of the air by their escape.—*Medical Times and Gazette, Feb.* 29, 1868, *p.* 228.

8.—ON THE PRINCIPLES OF TREATMENT OF DROPSY.

By Dr. George Johnson, Physician to King's College Hospital.

There are two objects to be aimed at in the treatment of dropsy ; these are, 1st, to remove, if possible, the original and exciting cause of the dropsy ; and, 2nd, to remove the dropsical accumulation. If we can accomplish the first of these objects, the second is generally attained with it. The dropsy will soon disappear with the removal of its exciting cause. For instance, the slight anasarca which occurs in chlorotic young women is a result mainly of the poor and watery condition of the blood ; and the dropsy quickly passes away when the quality of the blood is improved by nutritious diet, fresh air, and exercise, with the use of iron as a tonic, and perhaps an occasional aperient.

In the treatment of acute renal dropsy, it is important to bear in mind the relation in which the dropsy stands to the renal disease ; and it is especially interesting to observe the phenomena which occur during the progress of recovery. In particular, there is one phenomenon which deserves notice in connexion with the general pathology as well as the treatment of dropsy. I mean the copious flow of urine which occurs spontaneously during convalescence.

In cases of acute renal dropsy the urine is at first scanty and of morbid quality, being often high-coloured from admixture with blood, always albuminous, and usually containing numerous casts of the kidney-tubes. The scanty secretion of urine is the cause of the dropsy, and the secretion of urine is scanty because the flow of blood through the kidney is obstructed and the

structure of the gland changed, the tubes being filled with desquamated epithelium, and with blood and fibrin which have escaped from the gorged Malpighian vessels. Now observe what happens during the progress of cure in a case of this kind. The patient we will suppose to be placed in circumstances favourable for recovery: he is confined to bed; has a scanty diet; the loins are dry-cupped, or mustard and linseed poultices are applied there; and means are taken to excite the secretory action of the skin and bowels, and thus to lessen the work of the kidneys. Soon the secretion of urine begins to increase, until, in the course of four or five days, perhaps, the quantity of urine, which at first had been less than half the natural amount, becomes three times as great as the standard quantity, no diuretic medicine of any kind having been given.

The explanation of this spontaneous diuresis appears to be this. During the acute stage of the renal disease, the constituents of the urine, both solids and liquids, have accumulated in the blood, and have thence been effused into the areolar tissue and serous cavities. Now, urea itself is a most powerful diuretic; and no sooner is the inflammatory congestion of the kidney removed, and the freedom of the renal circulation restored, than the urea exerts its natural diuretic action on the kidney. The copious diuresis thus induced speedily removes the accumulated urinary solids and liquids from the blood, the areolar tissue, and the serous cavities into which they had been effused, and so the dropsy is cured.

This abundant flow of urine occurs without aid from diuretics or drugs of any kind. I have seen it happen while bread-pills alone were given as a *placebo*. Stimulating diuretics, such as squills, or cantharides, or turpentine, are injurious, by increasing congestion of the kidney. The best diuretics in such cases are means which tend to lessen the congestion of the kidneys; counter-irritation over the loins, especially by dry-cupping, hot-air baths and diaphoretics, purgatives, and a scanty diet.

In some cases of chronic renal dropsy, diuretics may be given without risk, but too often without much benefit in the way of removing or lessening the dropsy. A pleasant and efficacious diuretic is the imperial drink made with lemon, cream of tartar and sugar, with the addition of gin, in the proportion of a wine-glass to a pint. Hot-air baths often distress the sufferers from chronic Bright's disease; for the reasons which I have before given, the skin does not readily perspire, and the body consequently becomes painfully heated. Of late, therefore, I have been in the habit of prescribing for these patients a daily packing for two or three hours in a wet sheet and blankets. In this way I succeed in obtaining a more prolonged and copious diaphoresis.

and that, too, with less distress, if not with actual comfort to the patient.

In the treatment of cardiac dropsy, while we endeavour to remove the fluid by diuretics and by purgatives which excite copious watery discharges from the bowels, it is desirable to do what we can to sustain the power of the heart by nutritious food, stimulants, and tonics. Little or nothing can be done to repair a damaged valve, but much may be done to strengthen the muscular walls of the heart, and thus enable it to overcome the impediment to the flow of blood and the consequent tendency to dropsy which a diseased valve occasions. A combination of tincture of the perchloride of iron with tincture of digitalis is particularly useful in some cases of cardiac dropsy.

When other means fail to remove a dropsical accumulation, we may often afford great temporary relief, and prolong life sometimes for a considerable period by mechanical means—by tapping the abdomen, for instance, in a case of ascites; by acupuncture or incision through the skin of the legs for the removal of anasarca.

It is very interesting to note the phenomena which follow upon puncturing or incising the legs in cases of anasarca. There is first a copious drain of liquid from the punctures. Secondly, there is a further exudation of liquid from the over-distended blood-vessels; this liquid also escapes from the punctures or incisions, and its escape is often associated with temporary symptoms of exhaustion, such as a rapid and feeble pulse and pallor of countenance. Thirdly, there occurs often a copious secretion of urine, in consequence of a more free circulation of blood through the kidneys.

Dropsical accumulation tends to cause a secondary impediment to the circulation by the pressure of the effused liquid from without upon the blood-vessels. And, again, the capillary circulation becomes more and more impeded in proportion to the increasing distension of the veins, which results from cardiac or renal disease. The drain of liquid from the areolar tissue, allowing of a further exudation from the capillaries, thus removes or lessens the obstruction which results from over-fulness of the veins. The general circulation therefore becomes more free, and the greater freedom of the circulation through the kidney, is attended, as we have before seen, by a more copious secretion of urine. The greater freedom of the circulation through the kidneys is shown not only by the more copious secretion of urine, but also by the diminished amount of albumen, and not unfrequently when albuminuria has been caused by passive congestion, the result of cardiac disease, the albumen quite disappears for a time after a copious drain of dropsical fluid through the skin.

The free action of a hydragogue—elaterium for instance—is often followed by a copious secretion of urine. The gorged vessels are partly unloaded by the drain of liquid from the bowels; the circulation through the kidneys, as through other organs, consequently becomes more free; and hence a copious secretion of urine, and a rapid diminution or even a complete removal of the dropsy.—*British Medical Journal, March* 7, 1868, *p.* 215.

DISEASES OF THE NERVOUS SYSTEM.

9.—ON STRYCHNIA HYPODERMICALLY ADMINISTERED IN PARALYTIC AFFECTIONS.

By Charles Hunter, Esq., late Surgeon to the Royal Pimlico Dispensary.

[If morphia be proved to have greater rapidity and certainty of action when introduced into the cellular tissue than when given in the ordinary way, and that certain unpleasant symptoms can be avoided by giving it by this means, it is reasonable to anticipate certain advantages from the administration of strychnia also by the cellular tissue.]

Pereira tells us that, "of all diseases for which nux vomica has been employed, in none has it been so successful as in paralysis, and it is deserving of notice that this is one of the few remedies whose discovery is not the result of mere chance, since Fouquier was led to its use by legitimate induction from observations of its physiological effects."

One has to study the dose which the constitution of the patient requires more carefully with strychnia than some other alkaloids, and to begin with a minimum rather than a maximum dose, for the effects from a large dose may be almost too rapidly generated to be subsequently beneficial. Some patients, moreover, are far more susceptible of strychnia thus administered than others, but such is also the case with morphia and opium. In one patient, a lady who was suffering from injury to the spine and severe nervous prostration, after a fall more than a year previously, I found the one ninetieth of a grain of strychnia as large a dose as she could bear; on the other hand most patients bear well the one thirty-sixth of a grain, and some even the one twenty-fourth.

That this *mode* of administering strychnia is more powerful than others is shown by the experiments of the Hypodermic Committee upon rabbits, for they found that the one sixteenth of a grain of strychnine was the smallest dose that killed by the

mouth, the one twenty-fifth of a grain by the rectum, and the $\frac{1}{120}$th of a grain "by the skin" as they call it, but more correctly by the *sub*cutaneous cellular tissue. That committee give no experiments or observations of the therapeutic effects in man, but allude to the experience of Dr. Biegel, "who found one case yield to this method," and to my own observations in favour of its usefulness in certain cases.

I will now shortly detail a few cases of paralysis in which I have injected strychnine with benefit; at the same time I would remark that one must not look for the astonishing or suddenly beneficial results which seem to and do at times attend the injection of anodynes in cases of *pain*, of delirium, &c.

Strychnine should be employed as a nerve tonic or nerve-excitant in cases in which active irritation is supposed no longer to be going on about the clot or presumed cause of the lesion.

What we may fairly hope to expect when we inject strychnine is to find a *beneficial* result produced in a *shorter time* and from much *smaller doses* than when it is given by the mouth, and benefit may be expected even in cases which have, as in Case 1, been frequently treated, for weeks together, by its stomachic administration without benefit.

Case 1.—Hemiplegia of six years' standing. John C—, aged 52 years. Had always enjoyed good health, and followed his occupation in the brewery until six years ago, when, whilst at work he was suddenly seized with loss of power on the left side of the body. He has never since sufficiently recovered to permit of his returning to his occupation, and the utmost he can do is slowly by the aid of two sticks, to walk about dragging after him the paralysed limb. In addition to the want of power, he nearly always has *pain*, chiefly in the loins after walking a few yards, numbness and coldness, and a sense of weight in the limb, and fatigue from very trivial efforts, so that when out he has been accustomed to stand still frequently to rest, and to walk much bent forwards, chiefly to avoid pain in the loins.

This man had been a patient of mine at the Royal Pimlico Dispensary on and off for several years with these symptoms and loss of power over the bladder, and generally after a few months of stomachic administration of strychnia and other tonics, the symptoms have been moderated for a time. On the 24th of July, 1866, I put him on the one twentieth of a grain of strychnia—the acetate—with sulphate of magnesia, and some carminatives. This he continued for five weeks twice a day without improvement, except as regards the loss of power over the bladder.

On the 31st of August I began the subcutaneous administration of the alkaloïd, expecting little or no more effect, as so

little amelioration had attended the stomachic doses during three or four years.

To sum up briefly, this man, from August 31st to December 1st, that is during three months, had twenty-two strychnine injections; seventeen of these were in quantity the one thirty-sixth of a grain, the remainder were as strong as one twenty-fourth of a grain. During the five weeks which preceded the hypodermic treatment, he had taken as much as *three grains* of strychnia by the mouth without any apparent or palpable effects of the drug upon the paralysed limbs; *but direct strychnine* effects manifested themselves with the *first* as well as with *every* succeeding puncture.

Ten injections were given during the first three weeks. The results were:—1. Rapid improvement in walking power, and increased steadiness of gait. 2. The previous sensation of cold was replaced by a sense of warmth of both legs, chiefly of the paralysed one. This *induced* warmth has never quite subsided, and was always well marked for some hours after the puncture, in fact he almost *complained of* the heat the injections produced throughout his system. 3. The "heavy-weight" sensation in the leg was replaced by a sense of lightness of the limb. 4. The pain both of the loins and leg were removed.

These good results have continued, and he can now walk several miles, more upright, with comparative ease, with little or no pain, and by the aid, for a time, of only one stick.

The question might be put whether the three grains of strychnia previously given by the mouth did not materially assist in these results, seeing that by the cellular tissue he only had altogether about two-thirds of a grain?

My reply is simply that he had more than once taken strychnia for two or three months by the mouth without such good results. 2ndly. That the effects from the injection were direct and comparatively immediate. 3rdly. They were such as have been described spontaneously by other patients.

Case 2.—William J—, aged 60, of Belgrave Buildings. A tall, thin, active, intelligent engine-driver, was seized in Sept. 1863, with right hemiplegia. He was at engine-work when the fit came on. He became insensible, and had much fever, he says, for some days. He gradually improved so as to be able to get about with a stick, but the leg has very little power, feels heavy, and drags; he feels that there is no dependence upon it. The arm is weak, especially the muscles of the upper arm and shoulder, but he can grasp with his fingers.

December 9th. Omitted medicines by the mouth. I injected the one ninetieth of a grain of strychnine into the arm.

10th. He felt momentary pains in the course of the evening about the muscles of the forearm and heel; no muscular

twitchings, but less "stretchings" of the muscles. Repeated the injection.

11th. Felt a lightness and freedom in the muscles of the limb. Repeated injection of one ninetieth of a grain.

12th. Had a few slight muscular twitches in the lumbo-dorsal region on both sides of body. Injected the one sixtieth of a grain.

13th. The paralysed limbs feel stronger and lighter; has had a few twitches in the right side of the face, right arm, and both legs.

21st. Has had three more punctures from one seventy-fifth to one sixtieth of a grain. He had two or three punctures after this date, and by the end of the month the power both in the arm and the leg had greatly increased: he could turn his wife's mangle with the arm without pain or fatigue; he could stand steadily and strongly upon the leg, and stamp the foot on the ground with some vigour. On the other hand, during the ten weeks from the time of the fit to the time of the first puncture, *no* progress, of any note, had been made; whereas each puncture (save two) seemed to add fresh vigour to the muscles. More jerking of the muscles occurred after the last few than after the first puncture.

* * * * * *

April 24th, 1864. This man again came under treatment, not for paralysis, but for *cramp* of the *same* leg. For six weeks he had hardly passed a night free from four or five attacks of cramp which *came only* in the leg that had been paralysed.

I gave him a quarter of a grain of morphine subcutaneously; no cramp in the night; a slight twinge in the morning.

26th. Three attacks of cramp in the night. Repeated morphia injection.

May 24th. No cramp since.

October, 1866. Keeps well; able to turn the mangle, and carry parcels about, the leg remaining as strong as when the puncture was omitted.

Case 3.—James N., a sailor, aged 28. Came under my care on the 25th of May, 1866.

He had received a bullet fourteen months before in the dorsal region when at Shanghai. The bullet had entered close to the posterior border of the right scapula, and is somewhere deeply imbedded close to the spinal marrow. Paralysis came on gradually from that time; but it became much worse after he had been shipwrecked off Hong Kong eight months ago. He was two months in the hospital at the latter place. Strychnine was there given him by the mouth for some weeks; it caused jumping in the leg which is paralysed, but did not increase the power of the muscles.

He now walks very slowly and carefully with two sticks, and drags the left leg almost like a log; sensation is much impaired; leg still jumps occasionally, and would give way at once if he were to try his weight on it; has pain in the back and loins after walking many minutes.

It was doubtful to what extent power could be restored in this case if, as seemed probable, the bullet was mechanically interfering with the transmission of nerve power, the more especially as strychnine, as above mentioned, had already been given by the mouth *without effect.*

On May 25th, 1866, I injected the one twenty-fourth of a grain of strychnia into the cellular tissue of the upper arm, and on the 27th, the same amount into that of the leg.

28th. Can put the foot more firmly on the ground. Some jumping of the leg after both punctures; no jumping of the right leg.

28th and 30th. Repeated the same dose. Leg decidedly stronger and feels as if it now belonged to him, do longer like a heavy weight; can lift the foot higher, and he can now (after five punctures) just momentarily bear his weight on the foot without pressing on his sticks.

June 9th. I diminished the dose to the one thirty-sixth of a grain still injected every second, third, or fourth day.

29th. Can now walk two hours without feeling fatigue, and without that pain in the leg and loins which used always to occur after very small efforts. The leg now feels quite supple and "sensible."

July 23rd. Up to this date this man has had twenty-six injections from the 25th of May, just over two months, and altogether has had less than *one* grain injected during that time. The following are the chief results from the last few punctures. The one thirty-sixth of a grain suits him better than the one twenty-fourth of a grain; it causes much less jumping, at the same time it always causes the leg to feel lighter and looser, and as if life had come into the heavy useless painful weight it formerly was to him.

October 28th. He comes up once a week from Barnes to report progress. He can now easily walk three miles without stopping to rest, and without pain in the loins, and can walk across the room without his sticks. These good effects still remained when I saw him nearly one year after I had ceased to attend him.

Case 4.—Several injections of strychnia were given to a young woman who for some years had suffered from incessant muscular jactitations, frequent attacks of sickness, pain, and debility of the spine. The doses injected were from the one forty-eighth

to the one twenty-fourth of a grain. This patient had received great benefit from the atropia injection, and was not told that the agent injected was different.

The chief effects observed by her were a sensation of warmth in the spine, of greater strength and less pain in the muscles of the neck ; the sickness of stomach was also removed by it.

Case 5.—Mr. W., a publican, came to me on the 23rd of March, 1866. For the last three weeks he had complained of numbness of the legs and feet, of the sensation of pins and needles in the latter, and a fear of walking without holding or looking to see his feet. Is a sallow, puffy, unhealthy-looking man, temperate, bowels regular, urine often loaded with lithates, and difficult to pass at times, no albumen in it. Has the tight pain or band across the stomach. I put this patient upon alkalies, and the one twenty-fourth of a grain of strychnia twice a day by the mouth. He improved a good deal, sold his business, and went into the country.

On October the 23rd, he came back to London. He had been going on pretty well until three weeks ago, when his legs again stumbled and he could now only walk slowly and with difficulty. Urine again loaded with lithates, more loss of power over bladder, bowels, &c. I ordered him alkaline saline by the mouth for the state of the kidneys, &c., and this time gave him the strychnia subcutaneously, with the following results :— Decided increase of power of muscles of the legs and hips ; he could, after five punctures, get up from sitting on a low chair, without supporting himself as before with his hands. Usually feels a warmth of both legs, increased diaphoresis, "live-blood" as it were circulating in his thighs, generally also a stiffness, but no jerking thirty minutes after each puncture, less fear of falling when walking, diminution of the numbness and trembling of the legs, and increased walking power.

He had ten punctures altogether, some in the arm, and some in the thigh. The doses were from one forty-eighth to one thirty-sixth of a grain.

Case 6.—John B., aged 31. Came to me in March, 1866, with loss of power of the muscles of the left arm and forearm, consequent upon bruising and dislocation of the shoulder-joint some weeks before. Most of the bruise had disappeared, but the power did not return, as indeed it often will not for months; he could not grasp anything, nor pick up things. I ordered an iodine and camphor liniment for some days without benefit resulting, and then had recourse to the injection of strychnia.

This man had seven punctures ; three of which were into the muscles themselves, and four into the cellular tissue of one arm or the other. The chief effects observed by this patient were,

1st. He felt the influence usually in ten or fifteen minutes, viz., tingling in the fingers of the affected limb, some pains in, and increased temporary power of the muscles of the arm.

The power returned quicker in the muscles of the arm than did the activity to the finger.

He did not note any difference in the time in which the effect was produced, whether the injection was made into one arm or the other; but thought the fingers felt its influence most when the strychnia was injected into the pronator muscles. The dose in this case varied from the one forty-eighth to the one twenty-fourth of a grain.

Case 7.—William B., left hemiplegia. Came to me November 30th, 1867. Has had left hemiplegia for two years and a quarter, since a fit, which came on suddenly after two or three previous threatenings in August, at Madras; he was a soldier at the time. The left leg now drags, and he cannot walk (with his stick) more than fifteen minutes without the leg becoming stiff and painful; constantly has cramp in it at night, usually three or four times. The arm is very useless, and the mouth is still partially drawn; he seems very nervous, and frequently laughs when he should not; sleeps well, bowels regular, tongue clean, frequent micturition, urine healthy.

January 30th. Has had eighteen punctures in two months, with the following results:—Feels generally stronger, not only in the limb which is paralysed, but "in himself." Has much more control over his muscles. He looks less foolish, and hardly laughs now as before without occasion. Always feels "warmer all over" after each puncture; feels more power and warmth for some hours in the leg after the injections, which were all made into cellular tissue of one of the arms.

In this man it is the arm, contrary to my experience in the majority of cases of hemiplegia, which has received the most benefit; he can grasp much firmer with it, and hold weights which he could not when he first came; he is less nervous, and looks as if he now had some energy and will of his own; it is also interesting that the cramps in his leg which used, for many months, to wake him up at night, have never returned the last six weeks. This patient is still under treatment.

Remarks.—The effects obtained from the hypodermic use of strychnia are not so remarkable as are those which are constantly to be observed from the injection of atropia or of morphia. This is easy of explanation. Anodynes and narcotics are agents acting especially on nerves of sensation, and on nerve-centres, and are most valuable thus employed in acute affections, such as delirium tremens, acute mania, of intense pain, &c. Strychnia is a nerve-tonic, and nerve-excitant, and its employment is

chiefly valuable in cases of palsy and nerve atony, after all acute symptoms or causes have subsided. But because the case is one in which time is required to assist the action of the remedy, there can be no reason against trying to shorten the period required for the cure, or to limit the amount of the drug from which we expect benefit to accrue.

The chief advantages to be derived from the hypodermic use of strychnia are,

1st. *As a therapeutic test*, for three or four injections will almost always show if strychnia has any power or not over that particular palsy. If they have no effect it is almost useless to continue their employment.

2nd. *It is economical, both as regards amount of* (*a*) *alkaloïd to be used, and* (*b*) *time required for it to affect the system.*

(*a*) There is a great saving of the *amount of strychnia* required by this means over the stomachic employment of the same drug. The saving may be put at five-sixths of the amount which would be given by the mouth. For, as far as my observation goes, the one-thirtieth of a grain, injected every other day for thirty times, will consume just one grain of strychnia in sixty days: now if the somewhat ordinary dose of one-twentieth of a grain is given twice a day—and some physicians order the one-sixteenth and even the one-twelfth of a grain for a dose—it is evident that, in the sixty days, as much as six grains of strychnia are taken into the stomach. But, in cases of paralysis, where its continuance seems necessary, I do not, as a rule, inject the strychnia more often than twice a week, so that the saving is nine rather than six times less than the amount which experience shows us has to be given by the mouth for a reasonable amount of benefit to ensue.

Nor need this saving of material be considered remarkable nor the results singular. The experience of many medical men who have largely employed quinine injections in ague and fevers in India, Italy, and the West Indies, is quite in accordance with my results with strychnia. Dr. Moore, of the Bombay Medical Service, thinks four or five grains of quinine under the skin are equal to five or six times that amount taken by the mouth; and this opinion is confirmed by Drs. Chasseaud and McCraith, of Smyrna, by Dr. Desvigne in France; and more recently by Dr. Huggins and his colleague at San Fernando in Trinidad.

(*b*) In addition to the ultimate tonic effects of the strychnia being more quickly gained, and from a smaller amount of material, we have also manifested, by its introduction into the cellular tissue, certain phenomena indicative of the immediate or direct primary action of the agent upon the system. These are, 1stly, a general warmth or glow of the cutaneous surface. This warmth is experienced by nearly every patient, especially

those benefited by the injection. It is most felt in the paralysed limbs and down the spine, and at times making the patient "quite hot all over." This sensation lasts a few hours, according to the strength of the dose. 2ndly, a sensation of lightness and freedom in, and a removal of the "heavy-weight" sensation of the paralysed part. The leg often feels "light as a cork," and, 3rdly, increased temporary power which also generally begins in about twenty minutes, and lasts for perhaps some hours.

As regards the jerking of the muscles. This symptom of the action of strychnia appears to me to be of less frequent occurrence than when strychnia is given with a similar object in view for a lengthened period by the stomach. It seems practically to be the case, as might theoretically be conceived, that the primary tonic effects—namely, greater power and improved nervous sensibility—should follow the small strychnine injections, sooner and more effectually, than after the system has, by slow degrees, absorbed enough strychnia which has been administered stomachically, and the quantity of which so given has been perhaps three or four times as much as has really been necessary. When, on the other hand, muscular "twitchings," spasms, or cramp, have accompanied the palsy or numbness previous to the employment of the strychnine injection, those irregular muscular movements seem very quickly to yield to the tonic influence of the puncture, as in case 7, and in a bricklayer now under treatment.

Thus far, then, in favour of the hypodermic injection of this alkaloid. What, on the other hand, are the disadvantages of this method?

That great care is necessary lest too large a quantity should be injected of this agent, I need not point out: far better, therefore, always is it to inject a minimum than a maximum dose, say from the one-sixtieth to the one-eightieth rather than from the one-thirtieth to the one-twentieth. As a rule, however, the one thirty-sixth of a grain will suit most cases in which a tonic effect is desired upon a paralysed muscle or nerve.

I think it as well to mention that in two out of about twenty-five patients in whom I have injected strychnia a carbuncular state of the nose, or rather a collection of small boils arose. It is questionable to what extent, or whether or not, the strychnia injection is to be blamed. We know that belladonna will, at times, produce a rose-rash, and this is especially the case with atropia, its alkaloid, when injected.

Both of the patients who had the boils produced about the nose, had had a good many injections; one was the sailor with the gun-shot wound in the spine whose case is mentioned above; and the other was an Indian officer who had for years suffered from neuralgia and muscular prostration, consequent on—

according to Brown-Sequard — "congestion of the spinal marrow."

I have seen no ill results from the injection of strychnia such as have been described as following the injection of the acetate or sulphate of quinine. Those salts seem, in many cases, to have set up severe irritation, and subsequent sloughing of the cellular tissue, and even tetanus has been attributed in two cases to the injection of that alkaloid. Strychnia is, however, innocuous to the cellular tissue, and scarcely ever causes the least smarting, burning, or other local sign of irritation, when injected. Nor would quinine, as far as my experience goes, if some more basic salt than those above alluded to were used.—*British and For. Med.-Chir. Review, April*, 1868, *p.* 446.

10.—NEURALGIA: CASES TREATED SUCCESSFULLY BY THE SPINE-BAG.*

By Dr. John Chapman, Physician to the Farringdon Dispensary.

The cure of neuralgia, whether the disease be treated by drugs given internally, or by applications of various kinds at the seat of pain, or by the two methods conjointly, is confessedly almost always difficult, and in a large proportion of cases impossible. The cases reported below have been treated by a method altogether new. By stating each case with extreme brevity, I am enabled to present at one view within a small compass the results of several experiments; and I do this in the hope that they may produce such an impression on the minds of professional readers as may impel them to acquaint themselves with the pathological and therapeutical principles of which these results are an expression. I shall hereafter publish an exposition of those principles, illustrated by reports of cases *in extenso*, and shall then give a full description of the treatment adopted.

1. *Facial Neuralgia.*—T. H., a gentleman, aged 35, who had been suffering during the previous fortnight, requested my advice March 18, 1865. He was in great pain, which had been continuous from the previous day, and which had wholly deprived him of sleep. The pain was chiefly on the right side of the face and head; but during the morning preceding my visit the left side had become invaded. The affected parts were very tender, and somewhat swollen. The head was rather hot, the face flushed, the tongue thinly coated with whitish fur; pulse

* The Spine-bags are supplied by Messrs. Maw and Son, 11, Aldersgate Street, London, and can be obtained from them through the agency of any druggist.

92, full and strong. Several medicines prescribed by two physicians in succession had proved of no avail.

I applied a ten-inch spinal water-bag, containing water at 130° F. to the cervico-dorsal region, and shortly afterwards left the patient's room. Within half an hour I returned, when I found him asleep. The treatment was continued for two days by means of heat; afterwards I used ice (at first in the lumbar region), and from the time he first fell asleep he continued free of pain, which has not since returned.

2. *Facial Neuralgia.*—Fräulein S., aged about 25, consulted me February 3, 1867, on account of neuralgia affecting the infra-orbital and dental branches of the trifacial nerve. The pain was not confined to one side of the face, but was sometimes most acute on one side, sometimes on the other; it increased at night, and kept her awake the greater part of each night. She had been suffering in this way for about three weeks before I saw her. Her general health was good. The affected parts presented no trace of hyperæmia.

She was treated by means of ice, and experienced almost immediate relief. After three days of treatment she felt, and slept, very much better; and before the end of the fifth day the pain had wholly ceased. Nearly a year afterwards she told me that it had never returned.

3. *Facial Neuralgia.*—Mademoiselle M., aged 20, consulted me in August, 1867, when she was suffering from acute facial neuralgia, the chief foci of which were the infra-orbital foramen and the mental foramen of the right side. The extreme pain came on in fits, sometimes at 8 a.m., sometimes at 2 p.m., but between the paroxysms the face continued to ache, and at times the patient had pain at the back of the head. She had suffered in this way about a fortnight before coming to me, and had had several similar attacks during the preceding year.

The treatment consisted in the application of the double columned hot water-bag. The malady was immediately subdued: no distinct paroxysm occurred after the first application of heat; all pain rapidly and completely subsided, and since that date has not returned.

4. *Dental Neuralgia.*—A. W. B., a Russian gentleman suffering from dental neuralgia consulted me in September, 1867. The malady was chiefly confined to the teeth of both upper and lower jaw, but no particular tooth or teeth seemed to be especially affected. The pain was intermittent, and so severe as to interfere seriously with the patient's daily occupation. No cause of the disorder, which had continued some weeks, could be discovered, and the face so far from showing any sign of hyperæmia over the seat of pain, seemed cooler than normal. In the course of the first day of treatment by means of the spinal ice-bag, the

pain was completely subdued ; the cold was persisted in for some time, and during the remainder of the patient's stay in England he continued free from suffering.

5. *Dental Neuralgia.*—H. E., female, aged 21, suffering from violent and continuous pain, spreading over the teeth and gums of both the upper and lower jaw, consulted me, January 17, 1868. The pain was most intense in the lower jaw and on the left side ; she had intense headache also. The forehead and cheeks were notably hotter than normal, and she complained of great heat in the roof of the mouth as well as in the gums—which were swollen and sore. During the previous week she had had several teeth stopped with gold ; one of them became most especially painful ; and there was threatening of an abscess at its root.

The treatment consisted in the application of cold across the occiput, and of heat over the cilio-spinal region—in the first instance separately, and afterwards simultaneously. The pain was speedily and completely annulled ; it recurred, and was again annulled by the same method on several occasions. The patient volunteered the statement that, during the application of the heat, her mouth became perceptibly cooler.

6. *Facial and Brachial Neuralgia.*—Mary A. T., aged 44, first consulted me at the Farringdon Dispensary, December 28th, 1867, when she was suffering from neuralgia of the right side of the head, face, and neck, and along the right shoulder and arm —extending to the fingers. The right half of the tongue was also affected. The pain, which was exactly limited to the median line, was described by the patient—"like as if something is pulling the flesh off the bone, it's so dreadful, and sometimes as if the parts were screwed up in a vice."

She was treated by means of ice applied along the whole spine. She improved immediately and rapidly, and as early as January 15th, informed me that she had not had "a bit of neuralgia" during the whole of the preceding week. Up to this date (February 22th), the pains have not returned.

7. *Intercostal and Hypogastric Neuralgia.*—Miss E., who first consulted me, January 14th, 1865, complained of extreme pain on the right side of the lower dorsal and upper lumbar vertebræ, extending outwards and thence forward to the right hypogastric region. The pain had continued every day and night, with occasional intervals of three or four hours release from suffering, and had lasted about 20 years. The pain was so distressing and wearing that the patient's strength had become much impaired—especially by loss of sleep, and she was unable to undertake any regular occupation. The only sleep she could get was induced by brandy and narcotics. She suffered also

nearly every morning, and frequently throughout the day from nausea, and occasionally vomited "water."

I directed the patient to leave off brandy, opium, and morphia at once and entirely; and treated her chiefly by means of ice along the lower half of the spine. As early as February 14th, the patient was enabled to say to me,—"During the last few nights I have slept throughout the night without being disturbed at all—an experience quite newto me." On the first of the following April she informed me that she had already been many days absolutely free from pain. The nausea and vomiting had ceased; and the bowels had become "open every day—quite a new feature." I have since heard, from time to time, that this patient continues well.

8. *Intercostal Neuralgia.*—A man came to the Farringdon Dispensary, October 21st, 1867, and complained to Dr. Drysdale, whose patients I was seeing with him, of acute pain along one side of the chest; no assignable cause for it could be detected, and we regarded it as a case of intercostal neuralgia. I recommended the application of the spinal ice-bag along the dorsal spine twice a-day. "Sometime afterwards," as Dr. Drysdale related to the Harveian Society, "the man returned to the dispensary, looking so delighted," that the doctor "asked him what was giving him so much pleasure, when he replied that the 'ice-bag had done him a world of good: it had taken away all his pain with wonderful quickness.'"

9. *Intercostal Neuralgia.*—Mary B., aged 51, first seen by me November 27, 1867, complained of intense pain over the right shoulder, between the scapulæ, and along the right side of the chest. During the attacks of pain which came on and went away suddenly, the flesh, she said, quivered and tingled. She felt her body bent down as if she could not move it for fear of the pain.

She was treated by the application of the spinal ice-bag along the lower half of the spine during an hour twice a-day. The patient was surprised to find herself already quite free from pain during November 29th and 30th. Early in December, it recurred very slightly, but on the 18th, she said she remained free from neuralgia, except slight pain in the evening when fatigued by her work; she became much stronger, and was not so "nervous"; by December 28th all pain had gone, and February 26th, when I last saw her, it had not returned.

10. *Chest-Ache.*—October 21st, 1867, I was asked at the Farringdon Dispensary by Dr. Drysdale to prescribe for one of his patients, Maria Williams, aged 22, who was then suffering from continuous aching beneath the right clavicle, together with "dreadful pains" in the head, dizziness, heaviness in the

morning, and profuse leucorrhœa—which had lasted several months.

I prescribed the application of a narrow 16-inch ice-bag along the spine during 30′ twice a day—the bottom of the bag being placed on a level with the 4th lumbar vertebra.

October 26th. The patient reported that the ice-bag had done her "a wonderful deal of good"; that the leucorrhœa had stopped, and that *all* her head symptoms had vanished. I requested her to persist in the treatment previously prescribed. By November 2nd the pain beneath the right clavicle had quite ceased, and no one of all her other troubles, previously got rid of, had recurred.

11. *Mammary, Ovarian, and Uterine Neuralgia.*—H. M., a girl aged 17, who was first seen by me Feb. 24th, 1863, and who was suffering from daily attacks of epilepsy (*petit mal*), complained of extreme pain in the region of the left ovary, which was tender on pressure, and increasingly so immediately before the patient's menstrual periods; also of pain in the right mamma—which was considerably larger than the left, and of "fearful cutting pains" in the womb during menstruation.

I treated this case chiefly by means of iced-water applied along the spine. The result was complete cessation of the mammary, ovarian, and uterine pains; the mammæ became of equal size; and the epileptic vertigo, which had continued many years, ceased entirely a month after the treatment began, and has never recurred.

12. *Neuralgia of the Legs.*—I was consulted June 25th, 1867, by Mr. ——, M.P., aged 49, who complained of excruciating neuralgia in the legs. The pain was of the ordinary tearing, shooting, and stabbing kind, sometimes in one limb, sometimes in the other, and affecting one heel almost continuously. This was often so acutely tender that he could not bear to touch the ground with it. As a rule the patient suffered most at night, and could rarely get any refreshing sleep. The malady came on about sixteen years ago, and had recurred at intervals, ever afterwards. Until the morning he came to me he had never been free of intense pain for nineteen days. The patient was in the habit of drinking daily about a pint of sherry which I advised him to give up.

The treatment consisted in the application of a spinal ice-bag along the lumbar and the lower half of the dorsal vertebræ. The patient informed me, July 12th, that "practically speaking" he had had no pain since he began the treatment, and that he had slept well. I advised perseverance with the ice, and abstinence from wine. The patient wrote to me, August 21st,

"I continue to receive great benefit from the ice application, and shall continue it." The pain afterwards recurred in a mitigated form, but was greatly subdued by the ice, and, I incline to believe, would be quite cured if the patient would wholly abstain from wine.

13. *General Neuralgia.*—June 22nd, 1865, I was consulted by Lord ——, on account of neuralgia affecting different parts of the body: the walls of the chest, and the lower extremities were chiefly involved. The attacks generally came on suddenly, and lasted about fifteen hours, sometimes longer. The pains were of an acute, stabbing, and seemingly spasmodic character, and were often brought on by vigorous muscular exercise, especially deer-stalking, which his lordship is very fond of, but which his attacks either cut short or prevented him from indulging in.

I prescribed the application of ice along the spine, and five grains of citrate of iron and quinine during a few days at a time occasionally. When I saw the patient again, April 27th, 1866, he informed me that he had applied the spinal ice-bag as directed during several weeks at a time at three successive periods since he consulted me. He declared himself so greatly improved that he had been quite free from his malady for long periods together, that when it had recurred the pains had been comparatively slight, that he could resume violent exercises, including deer-stalking, without bringing on an attack, and that in his opinion, had he continued the treatment regularly for a longer time he should have been completely cured.

14. *General Neuralgia.*—Hannah E. complained to me January 4th, 1868, at the Farringdon Dispensary, of great pain in all the four limbs, but most especially in the fingers and toes, and in the left side over a spot about the size of half-a-crown; of headache each morning; of "dreadful" back-ache; and of excessive irritability of the bladder, involving the necessity of urinating about every five minutes.

She was treated by the application of the lumbar ice-bag 90′, twice a day. Within a week the pain in the limbs was almost wholly gone. By the 15th of January it, as well as her headaches, had quite ceased; her back was much better; and she was obliged to micturate only about every hour. On the 5th of February she reported herself free from every pain on account of which she had consulted me, and the irritability of the bladder was so much lessened, that she was only troubled with it at intervals of about 90′.—*Medical Press and Circular, March* 4, 1868, *p.* 201.

11.—THE INFLUENCE OF THE NERVOUS SYSTEM IN DISEASE.

By Dr. Samuel Wilks, Physician to, and Lecturer on the Practice of Medicine at, Guy's Hospital.

The cases which we may style nervous are without number—in fact, a very large part of all diseases which are called functional are indeed nervous. You have only to remember that every function of the body is regulated by nerve force, and you will perceive that the study of nervous diseases must embrace affections of every organ and tissue of the body. If this be so, their study must be considered to be still in its infancy. For example, a heart is beating irregularly, palpitating, or its force is being reduced; this, we know positively from experience and experiment, may arise from a nervous influence. But suppose that the heart should altogether cease to beat, and on post-mortem examination the dissector should pronounce it healthy—he means only as regards its muscular tissue and valves; but how about its nerves? Just think of those beautifully dissected hearts at the College of Surgeons, which look more like bundles of thread, so closely are they covered with nerves; and then remember the experiments which you have had shown to you of the movements of the heart when out of the body, as well as of the contraction of its segments which contain the ganglia; you will perceive the importance of the nerve supply to the heart, and the consequence of derangement or disease of its nervous elements. How many cases of dyspepsia, constipation, liver disorders, are due purely to a nervous cause! Think of the sympathetic nerve surrounding the blood-vessels! the vaso-motor nerves, and how they influence necessarily the supply of blood to the tissues, and thus exert a power over the processes of nutrition. Think of those large semilunar ganglia and the solar plexus with their supply to the vessels and viscera, and you cannot but suppose that their influence is all-potent. We get some kind of hint of the importance of these structures when, as we see in Addison's disease, all these nerves matted together and the patient die out from sheer exhaustion, while all the organs of the body are perfectly healthy. In peritonitis and strangulated intestine, also, we perceive a similar prostration. No wonder it has been suggested that in these ganglia lies the *fons et origo mali* known as cholera, since, in a few short minutes, a healthy body may be reduced to almost the condition of a corpse. I have very little doubt that the influence exerted by this solar plexus has much to do with the fact of the frequency of cancer in the neighbourhood of the stomach. Surely, then, we have much to learn about this important centre of our bodily solar system. Considerations

of this kind prompt me to the belief here yet may be that wide field of therapeutics open to us, in which we may employ remedies which may alter the nutritive process through the agency of the vaso-motor nerves.

No doubt such considerations have led to the belief that in the application of heat and cold might at last be found the universal medicine, the dilatation or contraction of the blood-vessels being thought sufficient to rule all other changes, and, these vessels being altered in calibre by the influence of the nerves, a depressant or stimulant to the ganglionic cells would be the means of regulating all nutrient processes. Speaking generally, there can be no doubt of the power for good or for evil of heat and cold. Consider the value of drugs from this point of view. Look at opium, which has so striking an influence on the nervous system, and then go into the surgical wards and see a large sloughing sore heal under its use; this will give some idea of the value of such classes of remedies, and how nutrition is modified under their use. I am beginning to use aconite more largely than I have hitherto done, seeing no *à priori* objection to much of the power which is attributed to it. I do not see my way to the arrest of a disease which is passing through its several stages, as pneumonia, but I see no objection to the belief in remedies of the opiate class having the power to arrest morbid processes. That aconite is used largely by homœopathists should not allow us to be prejudiced against its adoption. There is a story related of the late Rev. Rowland Hill, to the effect that he once demanded that a hymn should be sung to the tune of a then popular air. On a remonstrance by some of his hearers, he said he did not know why the devil should have all the good tunes. I must leave you to make the application.—*Medical Times and Gazette, Jan.* 4, 1868, *p.* 1.

12.—A CASE OF TRAUMATIC TETANUS TREATED WITH NICOTINE.

By Reginald Harrison, Esq., Surgeon to the Liverpool Northern Hospital, and Lecturer on Anatomy at the Liverpool Royal Infirmary School of Medicine.

[An additional interest has been given to this plan since the publication of the Rev. Prof. Haughton's paper on the counteractive effects of strychnia and nicotine. The following case of tetanus was in a boy 11 years of age, and resulted from an irregular scalp wound.]

Jan. 9th, 1857.—General rigidity of muscles increased. He was constantly crying out, being in great pain. He had not

slept during the night. The muscles in the perineum were extremely rigid, and it was necessary to remove the urine with the catheter. Though the muscular spasm was continuous, it was greatly increased in intensity on any one touching him, or even approaching him to alter his position. There was great pain about the diaphragm, with hiccough. Skin hot and dry; bowels only acting after an enema; pulse 120, and thready. There was a small collection of matter near the wound, which was opened, and about a drachm of pus evacuated.—Quarter to five p.m.; One-twelfth of a drop of nicotine was administered, the pulse being 120.—Quarter past five p.m.: The dose was repeated; pulse 108.—Quarter past seven: Pulse 112. One-eighth of a drop of nicotine was given.—Nine p.m.: The pulse was 68, and slightly intermittent. He was sweating profusely. There was much less muscular rigidity, and he was enabled to pass his urine naturally, which he had not done for thirty hours. The nicotine was discontinued for the night.

10th. Nine a.m.: The pulse had risen to 110. He had slept comfortably for several hours. Muscular rigidity certainly less than on the previous day. Skin moist and perspiring. No nicotine was administered during the day; but in the evening one twelfth of a drop was given, the pulse being noted at 132.

11th. Passed a tolerably good night, and remained comfortable during the day. Pulse 110. One-eighth of a drop of nicotine to be given at night.

12th. Had slept for several hours. Pulse 92, and regular. Towards the evening of this day he did not appear so well. The general muscular rigidity was much increased, and it was necessary again to make use of the catheter. Pulse 115. The nicotine was repeated (one-eighth of a drop) twice during the night.

13th. Half-past eleven a.m.; He had slept for several hours, and was decidedly better. Pulse 100. He was again able to pass his water naturally.

Without going more into detail, I may say that from this date his improvement was uninterrupted, and he left the hospital quite well in six weeks. The nicotine was given occasionally, being invariably followed with relief. During the whole course of the disease a generous fluid diet was given, without stimulants.

In this case, although symptoms did not show themselves till the thirty-second day after the injury, yet they were of a sufficiently aggravated character to lead me to apprehend a fatal result. The morphia in the first instance appeared to give relief; but on its failing, I fell back upon the tobacco treatment as a last resource. In administering the nicotine (which was procured for me with great promptness by Messrs. Clay and

Abraham), I found that one-twelfth of a drop was quite sufficient to produce a decided effect, and at no time was it increased to more than one-eighth, dissolved in alcohol and water. An hour always intervened between the doses. I mention these points for the guidance of those who feel disposed to try the plan of treatment. The effect of nicotine on the system is of so depressing a nature, especially in the case of those unused to tobacco, that it is most important not to give the least quantity more than is absolutely necessary.

The effects of the nicotine, as observed in this case, may be summed up as follows:—

1st. A diminution in the frequency of the heart's action. On the 9th of January, in four hours and three quarters, the pulse fell from 120 to 68. This was most marked on the first day; but the same effect was more or less invariably observed consequent upon its administration.

2ndly. The muscular spasm was invariably diminished. This was generally evident, but more especially by the voluntary discharge of urine, and by the breathing becoming easy and unaccompanied with diaphragmatic pain.

3rdly. A tranquillising effect on the nervous system generally.

4thly. It promoted the action of the skin, though I never detected the "snuffy odour" of the excretion as mentioned by others.

Such are the more prominent particulars in this case. I find there are many recorded cases where a recovery followed the use of tobacco; and some speak highly of its beneficial effects as an external application, in the form of a poultice to the wound by which the tetanic symptoms were produced. From the observation of many cases of this distressing disease, I should certainly feel more confidence in again adopting the treatment that I have recorded than any other I have seen tried.—*Lancet, Nov.* 9, 1867, *p.* 578.

13.—ON CASES OF TRAUMATIC TETANUS TREATED WITH THE ORDEAL BEAN OF CALABAR.

By Dr. Eben. Watson, Surgeon to the Royal Infirmary, and Lecturer on Physiology in Anderson's University, Glasgow.

[Dr. Watson has now treated six cases of tetanus with the Calabar bean—all successfully. The first case related in the following paper is that of a boy, 9 years of age, who had a contused wound on the sole of the right foot. Two days after admission the report is,]

At 7.45 p.m., of the 8th of June, the patient had a distinct fit of opisthotonos, which lasted for about five minutes. At 9.45

he had another similar fit, and during it he rolled over on his side. At 10.30 the treatment began with four drops of the tincture of Calabar bean, to be repeated every hour. He had several fits of opisthotonos during the night, and next day the dose was increased, first to five drops, and then to ten drops, every hour. He also had a drop of croton oil in the evening, by which time it is noted that there is no difficulty of swallowing ; pulse 96.

10th. The patient slept for periods of two or three hours several times last night, and therefore only took the tincture of the bean six times. At 4.20 a.m. he had a slight fit of opisthotonos. The bowels have not been opened by the croton oil, but he has taken food pretty well, even chewing a small piece of bread ; pulse 84.

11th. The treatment was continued yesterday, and he had only a slight fit at 6 p.m. He took a dose of the tincture at 8 p.m., and slept from that time till six this morning, when he had half a drop of croton oil in half an ounce of castor oil, and the doses of the tincture were resumed every hour. The patient is rather more rigid than usual this morning, and opisthotonic spasms are readily induced by the slightest touch of any part of his body. At 4.45 p.m. he still continued universally rigid, and the dose of the tincture was increased to fifteen drops ; but at 6, 6.45, and 8 p.m. the patient had fits of opisthotonos, the last fit being very severe, and lasting for about ten minutes. During the fits there was very profuse perspiration, and great rapidity of the heart's action. The bowels acted freely once, and the patient was able to take a little bread and meat-juice for dinner.

12th. The patient slept well during last night, but was roused every two hours to get the tincture of the bean. He had no fits, but occasionally started. This morning the muscles generally are much more relaxed than they have yet been ; pulse 80.—Evening : No fits to-day, but starts when disturbed. Has had fifteen drops every hour to-day. Bowels not opened.

13th. Had no fits last night, and slept well, but had his medicine every two hours. Seems improving this morning.—10 p.m.: The patient has had fifteen drops of tincture every hour to-day. To-night, when asleep, the jaw was completely relaxed, so that the mouth could be opened widely without force or waking the boy. The respiratory movements of the chest were likewise more natural ; pulse 68, weak, and somewhat irregular ; the pupils were very much contracted, but whenever the patient woke out of sleep he became rigid as before. The administration of the bean was suspended at 9 p.m., and at midnight he had half a drop of croton oil in half an ounce of castor oil.

14th. No fits last night. Bowels freely opened this morning. The tincture of the bean, in fifteen-drop doses, has been given every two hours till 8 a.m., and afterwards every hour. Pupils rather dilated. Could eat porridge, and even some bread, for breakfast this morning. At 3 p.m. the tincture was changed for that which had been used in a previous case, and as it was supposed to be stronger than that which the boy had been taking, the dose was diminished to six drops—a dose which had been effectual in the other case; and it should be remembered that the patients were about the same age, as well as under the same disease. This dose, however, proved insufficient in the case I am now relating; for the patient had a severe fit of opisthotonos at 1 a.m. (15th), and remained rigid for sometime thereafter. The dose was therefore immediately increased to eight drops every hour, but even that did not seem to produce the desired relaxation. The tincture formerly used was now resumed in the old dose of fifteen drops, and this was increased to twenty every half hour during the day and every hour during the night.

On the 18th the dose was increased to thirty drops every hour with good effect. During the last four days the patient gradually recovered from his relapse, apparently caused by the sudden diminution of the dose on the 14th. He had no decided fit of opisthotonos, except the one reported on that day; but he had for a time greater rigidity and opisthotonic starts. He is reported to be much more relaxed on the 18th than he had previously been; but it is noted that rigidity, especially of right limb, occurs when the wounded foot is washed, though that operation is performed in the gentlest way—simply by pouring tepid water over it. It is then laid in clean water dressing.

On the 19th, at 9 a.m., the patient being as above reported, the dose of the tincture was diminished to twenty drops every two hours; and he was ordered four ounces of sherry daily. At 3 p.m. he had two short fits of opisthotonos, and remained afterwards very rigid all over the body. The dose of the bean was therefore again repeated every hour during the day, and by the evening he was once more relaxed in all his muscles, and was able to separate the teeth about an inch. As his pulse was feeble he had then three ounces of brandy, which were given during every night for a time.

Next day he was again rigid and starting more readily, and this state continued during the 20th, though the dose of tincture was increased to thirty drops.

On the 21st, his bowels having been opened by croton oil, I ordered the tincture to be discontinued, as it did not seem to be sufficiently strong to combat the disease in this case, and I prescribed instead of it the alcoholic extract dissolved in spirit.

Of this he took a dose equivalent to one-eighth of a grain of the extract every hour, and after three such doses it is reported that he became much more relaxed; and at 10 p.m. he was ordered one-sixth of a grain every two hours during the night, to save waking him too often.

On the 23rd the patient continued to improve, and chewed a steak for dinner. Appetite good, but bowels again confined. He therefore had a dose of castor oil alone (half an ounce), and has continued regularly the doses of the extract prescribed on the 21st.

After this date he improved, though rather slowly, the muscles of the abdomen and back being the last in this case to give way. The dose of the extract was regularly given, and gradually increased till the patient took half a grain every two hours. This was from the 21st of June till the third of July, or twelve days. The effects produced were thorough relaxation of the muscles, occasional vomiting, without any great or at all events long-continued sickness—for the boy had a good appetite all the time,—and a noteworthy change as to the action of purgatives, which at first hardly acted, though strong, and by-and-by acted plentifully, though only of usual strength; latterly none were required at all. I think these circumstances show that, as the drug overcame the tetanus, it began to exhibit its physiological action on the alimentary canal—viz., a degree of irritation indicated by abundant secretion of mucus, and evacuation of both stomach and bowels.

On the 3rd July, therefore, I stopped the administration of the bean entirely, and ordered an increase of stimulants and gentle nourishment. Under this treatment the symptoms just mentioned soon subsided and disappeared. In a few days he could sit up freely in bed. He slept well at night, without even starting, and took his food with relish and ease. The wound of his foot had by this time nearly healed, and by about the middle of July he could rise from bed, and very soon ran about the ward as if he had never been ill. On the 30th he was dismissed cured.

It will not, I think, be questioned by any one that the preceding was an acute case of traumatic tetanus. The patient was admitted to the infirmary on the second day of the disease and he had already severe general convulsions. The administration of the bean was at once commenced by Dr. M'Gibbon, my house-surgeon, and henceforward the severity of the disease was mitigated, though in this case its progress was not at first very decidedly checked. The reason of this was twofold. 1st. The smallness of the doses of the bean. I wished to try if we could not counteract the tetanus without throwing the patient into such a state of alarming depression as occurred in my first case;

but we found that while these small doses mitigated and checked the violence of the fits, they did not altogether prevent their recurrence. Probably this was very much caused by (2nd) the severity of the disease in this case. I have elsewhere shown that the Calabar bean produces the exact counter-effect on the spinal marrow to the state in which tetanus originates. Hence when the latter is very well marked, so to speak, the former must be the more fully developed. Thus each case must be judged of pretty much by itself, and the doses of the bean proportioned to the severity of the disease in every individual instance. In the case just related the disease was, in my opinion, of a very severe type, and hence it was that the doses of the bean given at first only served to keep it in check. They were not powerful enough to conquer it altogether, and had they been neglected, the disease would soon have shown itself in all the distressing force so familiar to those acquainted with its phenomena. This statement, as well as the fact of the controlling influence of the bean, could not be better illustrated than by reference to the case itself. On the 11th of June the patient had a very long sleep, during which the drug was not given from 8 p.m. to 6 a.m., and in consequence the fits recurred, and the rigidity became worse. Again on the 14th and 19th, owing to the diminution of the doses, similar relapses occurred. Lastly, the duration of the case was much shortened by the increased strength of the dose towards the end of the treatment, when the extract was used instead of the tincture. Now all these relapses could not be mere coincidences with the diminution of the quantity of the bean taken by the patient. On the contrary, I think that unprejudiced readers will perceive a direct connexion between the diminution of the drug and the relapses, while the increase of the drug and the recovery are also most obviously related as cause and effect.*

It must be acknowledged that much has yet to be done in the way of procuring a perfectly reliable form for the therapeutical administration of the bean. In the recent edition of the British Pharmacopœia, I perceive that the powder of the bean and the alcoholic extract are the only preparations mentioned. In my earlier cases, and even for a long time in the preceding one, I employed a tincture made as recommended by Dr. Fraser, of Edinburgh, by percolation of the powdered bean with spirit; but I fear that this must be a preparation of very uncertain strength. And it was owing to this doubt that I changed it for the extract dissolved in spirit towards the end of the case just related. Eight grains of the former gradually rubbed down in and

* A case of traumatic tetanus has recently been reported which was treated entirely by hypodermic injections of tincture of Calabar bean. The result was quite satisfactory, and forms the seventh case treated by the bean, with only one death.

mixed with an ounce of spirits of wine, form a solution or tincture, every ten minims of which contain one-sixth of a grain of the extract. The dose can thus be easily regulated and given safely in solution. I do not, however, think that this tincture would continue of equal strength, as it by-and-by throws down a muddy deposit. I have not experimented with or otherwise tested this deposit; but I always had a fresh supply of the tincture sent to the ward every twenty-four hours. I believe that such a tincture as that now described is more certain and reliable than the tincture made by percolation; and, if it be found to retain its strength after filtration of the turbidity referred to above, it will be both as elegant and as permanent on keeping.

I shall now conclude with a short account of the fatal case referred to in the beginning of this paper.

On the 15th of September, about half an hour after midnight, a man, aged 30, was brought to the hospital in violent tetanus. The ball of his great toe had been slightly cut with a rusty nail about a fortnight previously, but was now nearly healed. The wounds were skinned over, though still red. He had been quite well, only very confined in his bowels, till two days before admission, when the jaw became stiff; and in due time he had general convulsions. These were much aggravated by his transmission to the hospital, so that when seen there by the house-surgeon of the ward into which he was brought he was in a very exhausted state, and steaming with perspiration. The fits recurred almost incessantly, and were very severe. He could not swallow either stimulants or the tincture of calabar bean. The latter was soon procured, but, from this obvious cause, its administration was unsuccessful.

When I saw the patient in the morning, about 9 o'clock, his pulse was very rapid and feeble, his face of a livid hue, and the tetanic fits were continual, and so severe that the man himself declared he was dying. I gave him half a grain of the extract of the bean dissolved in spirit and diluted with a tea-spoonful of water. He swallowed that with some difficulty, and, from his doing so, I hoped that we might yet gain time for the action of the drug; but in this I was mistaken, for he died in a severe convulsion which came on about half an hour after my visit.

When I heard of the great difficulty which this man had in swallowing, I at once thought of the hypodermic method of administering the bean. But I delayed carrying it out, because the man did take a tea-spoonful of fluid from myself, and I hoped he would continue to swallow a little, of the stimulants especially, until I returned, as I appointed with the house-surgeon to do after a short interval, to inject the tincture of the bean beneath the skin. I should have done so at the

nape of the neck, and I intended to have introduced ten minims in two portions. I regret that I did not try this expedient at my first visit, for I never had another opportunity. I may add, however, that the hypodermic administration of this drug is peculiarly applicable to such cases as present great difficulty of swallowing; for it is both certain and rapid in its effects. If another such case were to occur to me I should employ this practice at once without delay, warned by the present case that the patient's life may at any moment be cut short by tetanic spasm of the heart itself.

It can hardly be said that the bean was employed at all in this case, and so far as its operation is concerned it proves nothing. One lesson, however, may certainly be enforced from it, and that is the importance of commencing treatment with the bean in the earliest stage of this disease. The symptoms of that stage are quite characteristic and evident to one who has watched them but a few times, and no delay should be permitted before the administration of the bean. We should not wait even for the action of a purgative, desirable though that be, but ought to give the bean at once, and the purgative as soon as may be afterwards. It is only thus that we can have a good hope of success; for when once the disease has firmly established its power over the nervous system, it is difficult in all, and impossible in some cases to overcome it. This fact was well proved in some of my experiments with strychnia, related in the Edinburgh Medical Journal. When strychnia was first given and allowed to act, I never could restore the animal; but by giving even a fatal dose of strychnia, and at the same time a certain dose of the Calabar bean, I rescued the animal from the fatal effects of the former.—*Lancet, April* 4 *and* 11, 1868, *pp.* 434, 463.

14.—CASE OF GENERAL PARALYSIS SUBSEQUENT TO CHOLERA.

By Dr. Walter G. Smith, Dublin.

[The patient first passed through a severe attack of cholera, but a few weeks after recovery his attention was attracted by his lower limbs failing in strength. This increased rapidly until he was unable to stand without assistance.]

During the next week he rapidly lost power over all the extremities, became so helpless that he was unable to turn in bed, and if his feet were crossed, he was quite incapable of separating them.

His condition at that time was really distressing; completely paralysed, he passed some days in an almost vegetative existence;

motionless, not even possessing strength enough to draw the bed-clothes over himself, yet with his mind clear and active. He was much annoyed with severe tensive pain in the feet, which was completely relieved by friction, and the slightest movement or exertion caused intense pain and distress. At no time were the involuntary muscles seriously affected, though for two days, after he came up to town, he partially lost control over the rectum and retained the fæces with difficulty. He could always command the functions of the bladder, but the sensibility of the urethra was evidently diminished at one period, for he could scarcely feel the contact of the urine with the mucous membrane as it passed out of the body.

His urine, I regret to say, was not examined.

One of the most remarkable points connected with this case is the following :—

During a period of about ten weeks, for a portion of almost every day, he was unable to introduce the smallest particle, solid or fluid, into the stomach. Whatever he swallowed passed readily down the œsophagus, but near its termination seemed to be suddenly arrested by an impediment in, or constriction of, the passage, through which not an atom found its way. A few minutes after deglutition, part of the food regurgitated into the mouth, the act being attended with slight hiccup, and in a short time the whole was returned.

[Various means of treatment were adopted in the case.]

Finding the above means unsuccessful it was decided, at my father's suggestion, to have recourse to electricity, and the first trial of it was made on the 25th of last February.

The apparatus I used throughout the case was constructed by Messrs. Elliott of London.

It consists of a single Smee's element, the amalgamated zinc plates and the central platinised silver lamina, being about $4\frac{1}{2}$ by $1\frac{1}{2}$ inches each, thus exposing a surface of nearly seven square inches. They are excited by dilute sulphuric acid; one part of oil of vitriol to eight parts of water. The poles of the battery are readily put into connection with the thick primary or inducing wire of an upright induction coil, and the electrodes can be so arranged that the operator may collect at pleasure, either the extra current developed by the mutual action of the spirals of the primary wire on each other, or the more intense current induced in the fine secondary wire, or, if he wishes, can call into action a combination of these two currents.

The intensity of the induction stream is increased by the presence of a bundle of soft iron wires, in the centre of the bobbin, while the influence of this central temporary magnet is

regulated by withdrawing more or less of a copper envelope which closely surrounds these wires.

The necessary interruption of the battery current is secured by an ordinary trembler or rheotome, and the rapidity of the intermissions is governed by a screw which alters the distance of the trembler, from the central core of soft iron. The whole fits into a box 7 inches in height, 6·5 inches in width, by 4 inches in depth, portability and lightness being specially aimed at in its construction.

The slight inconvenience arising from the necessity of using acid with such a machine, as compared with the rotating magneto-electric machines is, I believe, far more than counterbalanced by the direct advantages resulting from the employment of a volta-electric arrangement, for

1. The latter can be worked without an assistant for hours at a time.

2. Its action is more uniform, and more easily regulated.

3. The current evolved from it does not cause so much pain, or such irregular muscular contractions as the magneto-electric current.

4. The ordinary magnetic machines are more liable to get out of order, become demagnetised after a time, and do not furnish nearly such a large quantity of electricity; and

5. The disagreeable noise produced by the movements of the wheels and armature of soft iron contrasts unfavourably with the quiet hum of the invisible little trembler.

I began with a gentle secondary current to the right arm, and the second day after directed the faradisation to the left arm.

It would be tedious to go step by step through the details of treatment, and I shall therefore present the facts in a condensed form.

As a rule, the patient was faradised three times a week, for about ten minutes or a quarter of an hour at each sitting. At first the secondary current was alone employed, but after a time the combined primary and secondary current was brought into play, which I have always found to exercise a considerably greater influence on the motor nerves than the secondary only.

The right and left extremities were usually excited on alternate days, and the total number of applications, extending over six weeks, was as follows:—Lower extremities about thirteen times, right arm about twelve times, and left arm about nine times. It is right to mention that for the greater part of the time during which the treatment by electricity was pursued, he took no medicine of any description, with the exception of a few diuretic powders (soda bicarb., pot. nitr. aa. gr. v.) for the relief of some temporary œdema of the feet.

A liniment of aconite and chloroform was rubbed in along the spine for a day or two, but beyond this no internal or external medication was resorted to.

The steps towards restoration to health were encouraging, and steadily progressive.

Very shortly after the employment of the induced current the arms began to gain in strength, and in about a fortnight he could stand with a little aid, though still with a tendency to fall forwards.

Gradually and slowly he regained the power over the lower extremities, and simultaneously there was a marked increase of susceptibility to the electric influence.

It was not till a week later that he was successful in moving forwards a step or two without assistance, and with his eyes shut.

Two marks of improvement he especially noted himself, viz.: that his hands lost the yellow hue which they exhibited in the early periods of treatment, and that he no longer suffered from cold feet. The circumference of the limbs increased rapidly, and within six days the right arm had gained to the extent of three-quarters of an inch. The calves of his legs by degrees resumed their natural dimensions, and on the 27th March he felt strong enough to drive out for a short time.

As soon as he was able to write firmly the faradisation of the arms was omitted, and on the 11th of April I saw him for the last time, and learned that he had walked two and a-half miles the day previously in an hour.

In conclusion, I would say a few words respecting the meaning of the terms to be adopted in speaking of the treatment by electricity,

By Electrisation is understood the employment of static or Franklinic electricity, as produced by frictional means or accumulated on a Leyden jar.

Galvanisation implies the use of the continuous galvanic current, as evolved from a voltaic pile or battery; and Faradisation expresses the local application of induced currents, whether caused by the mutual action of the spirals of a short and thick wire on each other, or by influence of a permanent magnet, or by a current circulating through a closed wire, and which admits of being rapidly opened or completed.

It is a fitting tribute to the memory of that great philosopher who has so lately passed from among us, that his name should thus be perpetuated in that branch of science in which he was chief.

Much of the discrepancy which is so often confessedly met with in the records of electro-therapeutics is, I am convinced, traceable to the carelessness and inaccuracy of writers with

reference to the form of electricity selected, and the precise manner of applying it.

Dr. Althaus, in his valuable work, gives some striking examples of errors arising from this source (see p. 163 and 343). Electricity is an agent potent for evil as well as for good, according as it is directed, and though when unadvisedly employed, it too often becomes an instrument of much mischief, yet, under judicious management, it is a tractable as well as a valuable servant, and it is especially remarkable as being one of the few potential remedies which the physician can command. —*Medical Press and Circular, Feb.* 26, 1868, *p.* 180.

15.—CARBOLIC ACID A CURE FOR TOOTHACHE.

[A correspondent writes to the Lancet as follows :]

Among the many virtues of carbolic acid is that of giving relief from the pain of toothache. I have tried it in a great many cases, and with invariable success. To one drachm of collodium flexile (B.P. 1867) add two drachms of Calvert's carbolic acid, full strength. A gelatinous mass is precipitated. A small portion of this precipitate inserted into the cavity of an aching tooth gives immediate relief. It may be kept in the cavity by means of a bit of lint dipped in the collodium.—*Lancet, Feb.* 22, 1868, *p.* 275.

DISEASES OF THE ORGANS OF CIRCULATION.

16.—ON THE PHYSIOLOGICAL DIAGNOSIS OF CARDIAC VALVULAR DISEASES.

By Dr. W. T. Gairdner, Professor of Practice of Physic in the University of Glasgow.

[No class of diseases requires more careful study and more care in forming both diagnosis and prognosis than does that of the heart. Most diseases of the valves are the result of endocarditis, but the amount, situation, and character of the lesion, as well as all the attendant circumstances of the case, must be taken into careful consideration. The study of murmurs, with great care and exactness, is necessary to preserve us from serious errors. A murmur always has a cause. It is a matter of reasoning in the particular case what that cause is.]

1. First, of *Aortic disease* ; in which, as you know, there is usually a very distinct murmur, single or double as the case may be, VS, or VD, or VS and VD, according as there is obstruction, or regurgitation, or both together. In aortic

disease you have the orifice through which the blood passes from the left ventricle into the arteries in some way or other interfered with ; and you might easily infer from this, that the tendency of aortic disease will be towards some kind or degree of interference with the systemic circulation. And in point of fact, it always is so to this extent, that in aortic disease sufficiently severe to derange the circulation at all, it is always the force and completeness of the systemic circulation that is first interfered with. But you must not estimate the force, or the completeness of the systemic circulation solely by the prominence, or apparent strength, or fulness, of the radial pulse ; nor can you estimate it accurately through the apparent strength of the heart's action. The reason is easily explained. In aortic disease, whether obstructive or regurgitant, when it affects the force of the systemic circulation, you invariably have a compensating hypertrophy of the left ventricle. I call it *compensating*, because the first effect of aortic disease being always to a greater or less degree to restrict the flow of blood through the arteries into the systemic capillaries, or, in other words, to diminish the tension of the arterial system generally, and thus to interfere with the distribution of the systemic blood, there is always a spontaneous impulse on the part of nature to overcome this difficulty. That spontaneous impulse takes the effect, after a time, of a habitually increased action and hypertrophy of the left ventricle. You may say that it is an almost invariable rule, that aortic disease, of whatever kind, tends to hypertrophy of the left ventricle ; and accordingly, if you find a murmur of some standing, which has given rise to serious symptoms without more or less evident hypertrophy of the left ventricle, it becomes rather probable that the murmur, however it may resemble that of aortic disease, does not, in fact, originate in the aortic orifice. And if you find a murmur, or combination of murmurs, associated with hypertrophy of the heart, but with the right ventricle predominating ; there might in such a case be aortic disease as one element, but it would be extremely probable that there was some other element, qualifying the aortic disease, and modifying its after-effects. Now, in aortic disease occurring alone, it often happens that the pulse is apparently not affected, or at least does not become feeble up to a very late period ; the only obvious effect that follows the disease is this relative hypertrophy of the left ventricle. Sometimes, though rarely, there are not even any serious symptoms ; a little palpitation, a little sense of vague uneasiness, sometimes a little tendency to faintness or giddiness, may be all that is observed. The explanation is, that there is in these cases only a comparatively slight amount of obstruction, and that it is under ordinary circumstances exactly met, or compensated, by the ventricular

hypertrophy. I have known a man suffer (if suffering it can be called) from such a slight form of aortic disease for a quarter of century, knowing, too, all the time that he had a cardiac murmur ; and yet, so slight has been the interference with the circulation, or so perfect the compensation, that he did not undergo any of the usual consequences of cardiac disease, and died at last of a different kind of disease altogether. This case is no doubt exceptional, but it was that of a medical man, and was very accurately observed. The usual fact, unhappily, as regards aortic valvular disease, is that it proceeds through a series of secondary changes, of which the hypertrophy of the left ventricle is perhaps the earliest in date, towards a fatal issue.

The tendency to diminished control of the heart over the systemic circulation is characteristic of aortic disease in a very special manner, as compared with other valvular diseases ; and the compensating hypertrophy of the left ventricle is accordingly a characteristic of the same group of cases. But the tension of the arterial circulation, and the pulse, are differently affected, according as obstruction predominates over regurgitation, or regurgitation over obstruction. The direct effect of aortic obstruction, supposing it to exist alone, is to make the pulse smaller in proportion to the degree of the obstruction ; and in extreme cases, where the orifice is reduced to a mere chink, this effect is actually witnessed, it may be said, of necessity. But as in moderate cases of obstruction the effect is modified by the hypertrophy of the left ventricle, it often happens that aortic obstruction does not produce a pulse appreciably smaller than natural. The degree to which the pulse is affected in simple obstruction is dependent on two circumstances ; on the one hand, the degree of the obstruction ; on the other hand, the failure of the left ventricle to maintain the full current of blood through the partially obstructed orifice. It is a complex problem in mechanics ; but the practical result is that, so long as the obstruction is moderate, and the left ventricle maintains its power, the pulse is little, or may be not at all, affected ; but when the obstruction is considerable, or when the left ventricle passes into the state of dilatation with diminished power, the pulse becomes small and feeble. But in aortic regurgitation the case is quite different ; and this is on the whole the more common, and by far the more dangerous, case. Here you have, as in the other case, a labouring ventricle, and a hypertrophied heart ; but the labour is in vain ; for at every impulse the ventricle sends a large quantity of blood into the artery, only to fall back again into the ventricle through the incompetent valves. The consequence is that, when regurgitation is extreme the artery cannot hold, cannot transmit, the blood it receives ; there is a large over-full ventricle, expelling at every systole a

great volume of blood into the aorta; but the smaller arteries are not fed, the systemic circulation is not profited thereby. The pulse is large and full, in accordance with the increased impulse and increased delivery of blood; but it is, at the same time, soft, undulating, and compressible, because the tension of the arteries is not maintained. The tension is not maintained, because the blood falls back towards the heart, instead of being passed on towards the capillaries. The arterial system is always unfilled, although the heart is working at high pressure, and the aorta is usually dilated. Such is the theory of the "pulse of unfilled arteries"; so accurately described by Dr. Hope as characteristic of aortic regurgitation. The pulse is large and full, because the impulse *a tergo* is increased; undulating and soft, because the tension of the artery is diminished. The expansion of the artery at each pulse of the heart is considerable; but the constant pressure, depending on the fulness of the whole arterial system, is much reduced in amount, and the capillaries, accordingly, are starved of their supply. In this state of matters the blood tends to accumulate in the left ventricle, and behind it. It stagnates, so to speak, within the heart. It does not get on. A great effort is made by the heart, but the force of the impule is lost in partially distending the aorta and larger arteries; it does not reach the smaller vessels; it does not fill the capillaries. Hence the patient is almost always pallid, anæmic-looking. To judge from his face and surface, you would say that he was bloodless; and the capillaries of the surface are in fact very imperfectly supplied with blood in such cases. But examine the heart, the lungs, the liver, and you will find that a large portion of the blood of the body is being heaped up there. The heart is gorged, over-worked; its pulsations are frequent, agitated, labouring; but it is the labour of Sisyphus; the burden always rolls back. It is difficult to avoid a somewhat figurative language here, but as I am satisfied that the figure is correct, I will pursue it further. The heart, thus worked constantly to the very summit of its power, or even beyond its power, has its innervation affected. It loses hold of its work; becomes *conscious*, as it were, that it fails to keep up the systemic circulation. The sensory nerves reflect these impressions through the ganglia, and the motor nerves respond by new efforts, but often by feeble and ill-sustained efforts. The heart *gets into a fright*, as it were, and is at the same time exhausted. Perhaps also its own nutrition is directly affected, through the inadequate circulation, and the inequalities of pressure in the coronary arteries. Then begins a new and terrible symptom. The patient gets anguished and distressed to a frightful degree. Often there is no positive pain, but in other respects the symptoms are those of *angina pectoris;* and

this form of heart-suffering is, I think, peculiarly, though not exclusively, characteristic of aortic regurgitation. The anguish gets written upon the countenance; you see that the patient has never a moment's ease; it is a look at once of distress and of exhaustion, which, once seen, is not easily mistaken. If not relieved, it is hardly probable that a patient can survive long in this state; yet I have known it last for days, and in a modified sense for weeks, during which little or no sound sleep has been obtained, though from sheer weariness the patient was always dropping off to sleep for a moment or two. He will tell you sometimes that he is *afraid* to sleep; and I have known a man in this state actually desire his attendant to keep him awake, for fear he should sleep the sleep of death. With all this, there may be no lividity; most commonly there is little dropsy; and it is only after a conaiderable time, and in certain cases, that there is hæmoptysis. A labouring heart, an imperfect systemic circulation, and the functional symptoms that I have endeavoured to describe; these, with the characteristic murmurs, form a typical picture of a case of aortic regurgitation. Now, although there may be sudden death in this state, or death from gradual exhaustion and sleeplessness, you are not to suppose that the majority, or even a large proportion, of patients with aortic regurgitation die suddenly. On the contrary, many of them survive in a very extraordinary way for a long period. The heart becomes enormously large; the left ventricle, from being hypertrophied chiefly, becomes dilated; the apex becomes much rounded, and the apex-beat diffused or undefinable; there is heaving movement over the whole precordial region. One of the consequences of this further development of the disease is that the mitral valve, which may have been quite competent hitherto, tends to give way; and you may find a mitral regurgitation-murmur (VD) superadded to the aortic murmur. There is not necessarily any deposit on the mitral valve; its incompetency in this case is purely mechanical; a result of the dilatation of the left ventricle. But another result, which either follows or precedes the leaking of the mitral valve, is increased congestion of the lungs, and often hæmoptysis; and afterwards you may have all the other sequelæ of disease of the heart in their most extreme development. Such is a description, open to correction, no doubt, in particular cases, but on the whole, I think, according to nature, of the course of development of aortic disease.

2. Let us now turn to *Mitral Disease.* In mitral disease you may have an AS murmur, indicating obstruction, or a VS murmur, indicating regurgitation; or you may have both murmurs together, and perhaps in a few cases a VD murmur superadded, indicating a complicated condition of obstruction and

regurgitation, as already explained. In mitral disease the systemic circulation is less directly involved than in aortic disease, and the pulmonary circulation more directly. Further, the evil effects of mitral disease, whether as regards the pulmonary or the systemic circulation, follow much more rapidly, as a rule, from regurgitation than from obstruction. Why is this? you will ask. Chiefly, I think, because the filling of the ventricles from the auricles is a slow process, which may, in cases of difficulty, extend over the whole of the long interval from the 2nd to the 1st sound. In cases of moderate obstruction, therefore, the filling of the ventricle, though it goes on more deliberately, and with a somewhat increased *vis a tergo*, is not necessarily very imperfect. I have known, indeed, the whole of this long interval occupied by murmur, showing that the filling was very slow, and yet the patient has appeared not much the worse. The auricle and great veins in such cases, no doubt, accumulate a larger reserve of blood; the tension of the pulmonary venous system is increased, and the auricle usually becomes hypertrophic; but, after all, the work is done, and done pretty completely; the ventricle fills at each diastole, or rather before each systole, and the circulation goes on. Hence, it is only in very extreme cases of mitral obstruction that you find after death, what perhaps you would expect to find in the majority of cases, a reduced capacity of the left ventricle. When this is the case, when the left ventricle becomes permanently atrophied in connection with mitral obstruction, it is evident that the obstruction has gone very far; that the auricle and great veins have not been able to throw enough of blood into the ventricle to maintain its full capacity and force. In most cases of mitral obstruction, however, the left ventricle preserves its size and capacity, and it may even, especially if regurgitation be also present, become enlarged or dilated. When mitral regurgitation, again, is present without obstruction, the left ventricle is almost always dilated, and often very much dilated. Here, as in the case of aortic regurgitation, there is a labour of Sisyphus; the ventricle is regularly filled at each diastole; it may be, overfilled. It then contracts and endeavours to empty itself; but the emptying is as much, or it may be more, towards the lung than towards the arteries; and both the pumonic and the systemic circulation suffer accordingly. Hence I have been accustomed to observe, and I think the observation is true in fact, whatever may be the theory, that even a considerable degree of mitral obstruction, without regurgitation, is much more easily tolerated by the system than a corresponding degree of regurgition, without obstruction. And conversely, among cases of mitral regurgition, unquestionably the least favourable are

those in which there is a dilated orifice, and the most favourable are those in which the orifice is contracted. It is scarcely too much, therefore, to infer that a moderate degree of obstruction in the mitral orifice, though, of course, always an evil *per se*, tends in cases of regurgitation through the same orifice to limit the amount of the regurgitation, and therefore to restrain the yet greater evils springing from regurgitation.

In mitral disease, then, it will depend very much upon the character of the disease whether the systemic circulation and the left ventricle will be much involved or not. If the disease be obstruction merely, and moderate in degree, there may be little or no detriment to the systemic circulation. If the disease be obstruction with regurgitation, the systemic circulation will be involved mainly in proportion to the amount of the regurgitation—the obstructive disease usually acting in such cases as a limit, or barrier, to the injurious effects of the other. In the case of regurgitation simply, the amount of the evil will reach the maximum, and will be proportionate, in some degree, to the amount of the regurgitation, and the degree of coincident dilatation of the left ventricle. In all cases, however, of mitral disease, whether obstructive or regurgitant, there will necessarily be a certain amount of increased tension, or over-distension, in the left auricle and pulmonary veins, owing to the increased resistance at the auriculo-ventricular opening, and the stasis of the blood behind the point of resistance. Whatever be the state of the systemic circulation, it is thus inevitable that in mitral disease the pulmonary circulation shall suffer. Permanent distension of the auricle ; dilatation of the veins and capillaries ; hypertrophy first, and finally dilatation with over-distension of the right ventricle—such is the order of events to be looked for in all cases of serious disease at the mitral orifice. The pulmonary circulation is performed in the face of a difficulty ; all the blood-vessels concerned in it become enlarged : an excess of blood is permanently lodged in the lungs, and it is transmitted through the distended capillaries at a slower rate than natural, and under an increased pressure ; the blood is thus less perfectly exposed to the air in the pulmonary vesicles ; cyanosis or lividity follows ; frequently the distended capillaries give way, and a peculiar form of hemoptysis, with condensation (hemorrhagic condensation, or "apoplexy" of Laennec) in the air vesicles, is the result. And these changes are invariably associated, sooner or later, with hypertrophy, and ultimately with dilatation of the right ventricle ; which in its turn, leads to increased tension and stasis in the systemic veins, enlargement of the liver, jaundice, dropsy of the limbs and serous cavities, albuminuria and secondary disorder of the kidneys, and often of the stomach.

In accordance with these changes are found certain modifications in the physical signs. The radial pulse, distinct and regular at first, may become small, feeble, irregular, or intermitting, or it may be almost lost at the wrist; these alterations corresponding either with regurgitation to a considerable extent, or with the last extremity of obstructive disease; and in the former case being usually associated with dilatation, in the latter with relative atrophy of the left ventricle. When the pulse is thus affected, it is not uncommon for the murmurs to become indistinct, or even to be lost; partly because the circulation is nearly brought to a stand, and partly because the dilated heart does not transmit the sounds so readily as before to the site of the apex-beat. In many of these extreme cases, in fact, the apex-beat is lost, or exchanged for a diffused pulsation corresponding with the dilated right ventricle. Unless in extreme cases, and almost always in cases of moderate obstructive disease without regurgitation, the apex-beat remains distinct, often preternaturally distinct. It is in such cases that a sign is usually observed which may be regarded as the first note of undue tension in the pulmonic circulation—the loud, sharp, jarring *snap*, felt as much as heard, which takes the place of the normal second sound over the pulmonary artery. This phenomenon is much more common, and much more highly developed in obstructive than in regurgitant disease; and it is observed only when the heart is acting strongly and regularly, without any considerable dilation of the cavities, or weakening of the fibre. When dilatation occurs, the organ is *broadened* rather than *elongated;* the dull percussion is extended transversely, and both to right and left; the apex is ill-defined, the sounds are muffled and low pitched. In such cases you may expect the murmurs to become obscure, and the pulse feeble and small as the dilatation proceeds; occasionally, too, you will observe a very remarkable change in the position, and sometimes also the character of the murmur; the explanation of which is, that while the mitral murmurs are becoming obscure, and are thrust into the back-ground, as it were, owing to the dilatation of the right ventricle, a new murmur is arising in that ventricle itself, one of tricuspid regurgitation. Indeed, tricuspid regurgitation in this case bears to mitral disease almost exactly the same relation as mitral regurgitation sometimes bears to aggravated aortic disease; it is the witness of a ventricle dilated and overpowered to such a degree as to cause leaking of the valve.

3. *Tricuspid disease*, which we may take next in order, is rarely otherwise than of secondary origin, *i.e.*, the valve is rarely thickened, the orifice rarely obstructed by deposit. Even in museums, preparations of tricuspid disease in this sense are

rare ; and in museums their frequency is apparently overstated, as it were, from the fact that preparations of unusual lesions are much more apt to be preserved than of common lesions. In *post-mortem* examinations, continuously and carefully pursued, it may probably be assumed as a datum that tricuspid disease will be found in about one in a dozen cases of disease of the valves ; meaning by disease, of course, in this instance *obvious* disease, *i.e.*, thickening, contraction of orifice, or obstruction by deposit. And, further, in all but an inappreciable proportion of these cases, the disease occurs in combination with mitral or aortic disease, or both together ; so that tricuspid structural disease, absolutely alone, may be said to be one of the rarest of pathological phenomena. But although tricuspid structural change is rather rare—very rare indeed, as a primary and uncomplicated form of disease—tricuspid regurgitation is by no means rarely met with in practice ; partly because it is one of the forms of structural change most commonly associated with mitral disease, but still more because *incompetency* of the valve may occur, and probably often does occur, without structural change, chiefly in consequence of dilatation of the right ventricle. Speaking generally, you may say that, in the immensely greater proportion of cases in which it occurs, tricuspid regurgitation is a secondary phenomenon. It is a *leakage*, not a *deformity*, of the valve ; and it is a leakage depending commonly on dilatation of the right ventricle. Therefore, speaking still in general terms, the causes of tricuspid disease (for obstruction of the orifice may be dismissed from consideration at present as extremely rare) are simply those of dilatation of the right ventricle ; and among these we have seen that mitral obstruction and regurgitation assume a very high importance. It has never been quite clearly shown whether tricuspid regurgitation is, or is not, always attended by murmur ; but, as a matter of fact and experience, I can assure you that it is not very uncommonly attended by murmur ; and that the murmur of tricuspid regurgitation is associated in a large proportion of the cases in which it occurs with that of mitral regurgitation, or perhaps still more frequently with that of mitral obstruction. To detect this association it is necessary, of course, to be very careful ; for the murmur of mitral regurgitation closely resembles that of tricuspid regurgitation, and in some cases the diagnosis absolutely cannot be made at all except as a matter of inference, and with a chance of error. But in other cases the diagnosis is not at all difficult, if you carefully observe the rules and principles laid down in a former lecture ; and the certainty I have that this distinction can be made very accurately in some instances, while in others it is difficult or impossible, has led in my mind to a belief that the mitral and tricuspid murmurs have

frequently been confounded by auscultators; or, to speak more correctly, the latter have been frequently overlooked. The cases in which the distinction is most easy are those in which there is a mitral obstruction-murmur with tricuspid regurgitation; the A S murmur being heard at one point very distinctly, and the V S murmur equally distinctly, but at quite another, and more or less remote, point of the chest; the first at the apex only or chiefly, the second only or chiefly over the right ventricle. It is, however, quite possible in some cases to make out the tricuspid regurgitation-murmur, when occurring along with the mitral regurgitation-murmur only; and, what will strike you as more problematical, it is possible, though difficult, to distinguish in the same case mitral obstruction and regurgitation from aortic obstruction and regurgitation, and tricuspid regurgitation from both of these. This I have proved by examination after death in more than one instance.

The only other determining cause of tricuspid regurgitation-murmur which has sufficient clinical importance to be noted here, is emphysema of the lungs. Speaking in a practical sense, almost all that I know about tricuspid murmurs is their association with mitral murmurs on the one hand, and with emphysema on the other. And the nature of this association is not doubtful; it depends evidently on the tendency to dilatation of the right ventricle which accompanies emphysema of the lungs in many cases. Indeed, the chief difficulty in this matter is not to explain the presence of the murmur, but to account for its absence in cases in which a murmur might reasonably be expected to be present. For it is quite certain that you may have emphysema of the lungs, and widely dilated right ventricle such as almost necessarily implies tricuspid regurgitation without any murmur at all.

It is common in books to describe pulsatory movement of the veins in the neck as characteristic of tricuspid regurgitation. This, I am almost sure, is not correct. Pulsation of the jugular veins is, no doubt, frequently associated with tricuspid regurgitation, inasmuch as the latter depends usually upon dilatation of the right side of the heart. But by far the most striking case of exaggerated venous pulse that I ever saw was one in which I am much disposed to think there could have been no considerable tricuspid regurgitation, though there was a very manifest obstruction at the tricuspid orifice. In this case the pulsation must, I think, have been due to a dilated and thickened right auricle. A lesser degree of pulsation in the jugular and innominate veins is frequently observed in a state of health, or at least, in the entire absence of anything like cardiac disorder; and, on the whole, I have come to the conclusion, from this and other experience, that venous pulse is not to be

depended on as an indication of tricuspid regurgitation. Its presence or absence in any particular case seems to depend more on the competency of the venous valvular apparatus than on any other cause; although, of course, the closure of the venous valves is more likely to be imperfect with a dilated heart, and pulsation will be apt to be more distinct in proportion as the veins are permanently dilated, and the valves incompetent. In many cases of very considerably dilated right heart, however, I have observed that the venous valves remain nearly competent, even when the veins are much distended, and in such cases there is of course little or no pulsation.

The other symptoms of tricuspid regurgitation are altogether those of dilated heart, and it is not necessary to enlarge upon them at present.

4. Obstructive disease of the right side of the heart, whether of the tricuspid or the pulmonic orifice, is, as I have already said, rare under all circumstances, and exceedingly rare as a primary form of disease. I can tell you very little about either, except what results from the observation of isolated instances, and from others recorded at intervals in medical journals. I have seen personally, I think, a few instances of primary disease of this kind; one, almost certainly, of tricuspid obstruction, and perhaps two or three of pulmonic valvular disease. Strange to say, although in these cases the physical signs were extremely intense and characteristic, the symptoms were of remarkably moderate intensity, so much so that two of the cases have been under observation, more or less, for years, with but little alteration, if any, towards the worse; and a third was kept in view for some time, but has since been lost sight of. There is, of course, a risk of fallacy in all such incomplete observations, and I do not ask you to found any conclusions upon them. The general course of experience goes to show that obstructive disease of the right side of the heart is usually attended by very marked lividity, or cyanosis; so much so as to form a distinguishing character of this form of disease. In two of my cases, just mentioned, there was *occasional* lividity to a considerable extent; in the third, none. In other cases (mostly, however, complicated and fatal), I have seen the characteristic cyanosis well developed; but, on the other hand, in one remarkable case, of which I witnessed the dissection many years ago, there was scarcely any lividity, although aortic, mitral, pulmonic, and tricuspid openings were all very much obstructed by chronic disease. In this case I can tell you nothing about the physical signs, as they were not observed, and the patient, indeed, was hardly known to have had heart disease at all.

5. Exocardial, anæmic, and aneurismal murmurs are very confusing, and are apt to lead to great errors of diagnosis, unless

the physician is guided by a thorough and exact knowledge of their typical characters, and also of all the collateral circumstances likely to be associated with them in particular cases. It may be useful for you to note the following particulars, which are all that can be introduced into the present lecture. Exocardial murmurs more commonly resemble those of tricuspid and mitral, aneurismal and anæmic murmurs those of aortic disease. Exocardial murmurs are very commonly double, but not *exactly* rhythmic, nor blowing in character; anæmic murmurs are almost invariably single, VS in rhythm, and audible chiefly near the base and over the vessels; aneurismal murmurs are more commonly single than double, usually VS in rhythm, but sometimes also VD; and, as they are apt in this last case to be associated with the murmurs actually arising from valve-disease, the diagnosis is often of great difficulty and nicety, requiring much patience and skill, and a very comprehensive survey of all the available facts. The same remark applies to certain difficult and complex cases of disease to which no rules can be applied but those arising from accurate knowledge of principles and much clinical experience. I have attempted on the present occasion to convey to you only the first principles of cardiac diagnosis; but, while no department of our inquiry has been nearly exhausted, I think you will find also that no subject has been treated in so superficial a manner as to conceal from you the real difficulties of the questions that arise at the bedside. I trust also that you will find some degree of assistance from the form which our inquiry has naturally assumed in the course of oral delivery.—*Glasgow Medical Journal*, *Dec.* 1867, *p.* 283.

17.—ON A CASE OF EMBOLISM OF THE RIGHT VENTRICLE OF THE HEART.

By Dr. R. Richardson, F.G.S., &c., Rhayader.

By Embolism (ἔμβολος, *a wedge* or *stopper*) we understand that a vessel has been obstructed by a fibrinous clot, which might have formed in the heart or in the circulatory system, either arterial or venous, and by impulsion forced into a vessel so as to obliterate its cavity completely. The consequence attending such an accident depends upon the importance of that vessel to the function of life. When it occurs in the pulmonary artery, death is almost instantaneous; if the vessels of the brain, it causes apoplexy and softening of the brain; but if the arteries of the limbs are affected with it, gangrene of the part is the result.

These concretions are sometimes found in both sides of the heart, but more frequently in the right. Authors differ in

opinion as to the mechanism of the formation of these clots. Some hold that the arrest of circulation and stagnation of blood in the heart is the cause; others, that inflammation of the internal membrane of the heart, valves, or vessels is the primitive cause of them. But I am inclined to believe, from the following case, that the cause in some cases must be in the blood itself, as there is no history of any previous illness whatever up to the day of death.

On Sept. 24th I was called to see Miss T—, aged eight years and a half, who was taken suddenly ill. On my arrival I found her in bed constantly spitting, and sometimes vomiting, some frothy mucus; her countenance looked pale, eyes sunken, and no pulse perceptible at the wrist; the action of the heart was tumultuous; very deep inspirations; feet and legs icy cold. I could at once see the serious aspect of the case. She did not complain of any pain, only of something in her throat. She turned over on her left side herself, and, with a deep inspiration, expired, as if with a sigh. She possessed her faculties to the last, and was dead in about ten minutes after my arrival, and all that time I was present.

The history of the case is as follows. She had been to church on the Sunday, and on the Monday she was about as usual, but complained rather of headache. On the Tuesday (24th) she complained of a great quantity of phlegm coming in her throat, and when she moved it produced a vomiting; nothing but phlegm, which was very tenacious and frothy, came up. She was carried to bed about three o'clock p.m., and died about six the same evening, having been about the house in the morning.

The question naturally arose, what could have caused such rapid death? She was very tall for her age, and of a fair complexion, clear skin, and altogether she had a tubercular diathesis. Her mother died suddenly when my patient was about a fortnight old, after having suffered nearly a fortnight from phlegmasia dolens. I have no doubt an embolus was carried by the circulation into the pulmonary artery, and that accounts for so sudden a death.

As there was no reason for suspecting foul play, I could not very well account for so sudden a death in a child, as there was no symptom of any previous disease of the heart or of the brain, So, on the Friday, in conjunction with my friend, Mr. Rowland, F.R.C.S., of Strata Florida, who kindly assisted me, a post-mortem examination was made.

On inspection of the body there was nothing particular to observe; the body was well developed and very tall. On opening the abdomen we found all the viscera healthy; the stomach was healthy, and contained about an ounce of dark-coloured

fluid; the spleen was congested, and the liver rather large.—Chest: The pleura and pericardium were healthy; the lungs were very much congested, and the bronchiæ were full of frothy mucus; the left lung contained miliary tubercles. The heart looked normal. On opening the left ventricle, it was found to contain only about a drachm of dark blood; all its membrane and valves healthy; the auricle also healthy. On opening the right ventricle we found an embolus, about two inches and a half in length, an inch and a half in breadth, and about half an inch in thickness, which plugged completely the pulmonary artery. It was not adherent to the membrane nor the valves of the heart; its colour was pale-red, much like the appearance of uncooked veal. The right auricle was healthy. The head was not opened.

The case must be considered as quite unique, coming on without any apparent anterior cause. As we find the clot perfectly free from any adhesion to the heart or its valves, we must conclude that it was of recent date, probably not more than twelve hours, as when it commenced to develop it must have caused a certain amount of trouble in the system, which we do not find here before the fatal day. It must have been caused by the state of the blood itself—from the dyscrasia of the tubercular state as containing some morbific material. My namesake, Dr. Richardson, attributes the fluidity of the blood to the ammonia it contains; but it yet remains to be proved whether, in a case like the one above narrated, embolism occurs from lack of that substance.—*Lancet*, *Nov.* 23, 1867, *p.* 641.

18.—ON INTERMITTENT PULSE AND PALPITATION.

By Dr. Benjamin W. Richardson, F.R.S.

[Intermittency of the pulse should be taken as a failure entirely confined to the action of the left ventricle. The ventricle does not contract when stimulated by the arterial blood contained in it, and then by a prolonged thud makes up for the beat missed. What is the cause of this frequent affection?]

We should naturally begin by looking into the heart itself for a cause. We should now be wrong. The fact alone that during the intervening periods of intermittency the action of the heart is natural, would go far to indicate that in it there can be no great amount of organic lesion. Still this of itself would be little were it unsupported by more direct evidence. Being greatly interested in this matter, I seized once the opportunity of examining after death the heart of an aged man who for many years presented the phenomenon of intermittency more determinately than I ever before had seen: his pulse

never, as far as I could learn, failed to intermit less often than once in eight beats. His death was from senile decay, but his circulation may be said to have outlived all the other of his systemic powers. When quite insensible, the pulse, with long hesitations, came up again, and the pulse was beating at the end even when the respiration had ceased. And so after death, instead of a diseased heart, the heart was found the healthiest of the organs. There was no trace of valvular disease. There was no departure from the natural size and condition of the cavities or the thicknesses of the walls ; the coronary arteries were normal, and the muscular structure, quite free from fatty and granular degeneracy, was merely, as the tissues are in the aged, a shade paler than is common in the young and robust. Since the occurrence of that case I have confirmed the experience then gained by three other experiences ; and I feel bound to say from what I have seen, as positive truth, that the most marked intermittency of the heart may be present without evidence of any known form of organic disease of the organ itself ; and as in this case one fact carefully assured is as good as a thousand, I think we may accept that there is no known morbid condition of the heart itself, structurally considered, that produces the phenomenon of intermittent action. Intermittency may coexist, of course, with other signs of cardiac derangement essentially of structural origin ; a fatty heart may intermit ; a heart with faulty valvular mechanism may intermit ; and intermittency with structural change may form, and often does form, a most serious complication. This we must at once allow, but we must allow it feeling that the intermittent action has no necessary connexion with the structural disorder, but is evoked by a cause remote and independent. Pre-existent diseases of a special kind, such as acute rheumatism, do not, so far as I can learn, leave intermittency in their train ; neither, as far as I know, is the phenomenon more common in those who have structural disease of the heart than in those who have not.

Intermittency is not peculiar to either sex. Intermittency is not governed by age exclusively. In one instance I noticed it in an infant on the day after its birth, and it continued in him in the most marked degree for five years. It then gradually passed away. A medical friend once brought me one of his children, a boy five years old, who had the symptoms in the most intense form, so that his parent was seriously alarmed—the boy himself, however, not being conscious of any ailment. In this case also the symptom has disappeared, the boy being now nearly twelve years old. Looking over my notes of cases, I find, in short, that there is no particular age at which I have not met some persons who have intermittency of the pulse. But this must be admitted at the same time, that the symptom

is most frequently seen in persons of advanced life ; in fact, in very aged people the absence of it is the exception rather than the rule. It is by no means unfrequent in persons of middle age, and it is as common in those who are prematurely as in those who are veritably old. It is least frequent between the ages of ten and thirty.

Intermittency of the pulse is not peculiar to the human species. A neighbour of mine had an old Italian greyhound which presented the phenomenon in the most singularly distinct form. I also had a dog that presented the symptom ; this animal was not young, but hearty and disposed to fat and somnolency.

I shall probably assert what many will not agree with when I state that, according to my experience and inference, intermittency of the heart has no relation to what is commonly called dyspepsia. It is true that many dyspeptic persons have intermittent pulse, but this fact does not affect the question, because it is equally true that many persons who have determinate intermittency of pulse have the most keen and excellent digestion. I have a patient at this very time whose case is strictly in point : his pulse intermits every sixteenth beat, but his tongue is clean, his urine natural, his appetite good, his sleep sound, and his bowels regular. After taking food he has no pain, he has no flatulency, and, according to his own often repeated expression, "he does not know that he has a stomach." On the other hand, we constantly see dyspepsia in all its varied and severe forms without the sign of intermittency.

I know of no diseased condition of the blood with which the phenomenon of intermittency is connected. Neither have I been able, after careful research, to trace it, in the light of effect from cause, to any affection of the lung, the liver, the kidney, or other secreting or excreting organ.

Thus we are driven at last to one sole system of the body in which to seek for the origin of the phenomenon of intermittency of the heart ; and that is the nervous system. Followed to this seat, all the evidence is too unequivocal to be doubted. The suddenness of the phenomenon, its purely functional character in so far as the heart is implicated, and the other symptoms by which it is attended, leave no room to doubt the correctness of the view that the momentary cessation of the left ventricle occurs from deprivation of the force which it should receive to enable it, under the stimulus of the blood thrown into it by its auricle, to contract and regulate the blood current in its arterial course.

All the evidences, again, point to the fact that, in every case of true intermittency, one particular point or centre of the nervous system is the primary seat of the derangement. The

phenomenon is too uniform to admit any explanation less definite ; it speaks to us and says there is deficiency of force in the centre of the nervous system which provides for the ventricular contraction on the left side of the heart, or there is some centre which balances that supplying centre, and which is over-active.

The derangement might be in the ganglionic centres of the heart itself, but if it were, the nutrition of the organ would surely be more decidedly influenced, and the symptoms would be confined to the heart. The derangement might be from irritation in the periphery or in the line of the great nerve trunks, as the pneumogastric ; but if it were, it would hardly be so persistent for years and evolve no other signs of muscular disturbance. Where then is the primary mischief ? I believe it to be in the cerebrum. The clinical history of every case I have seen points to that seat. In the aged it is an invariable follower of failing cerebral power ; in the very young it presents itself with other indications of cerebral derangement. But that which impresses me most in favour of the cerebral origin of intermittent pulse is the mode in which it appears in the prime of life. I have never met with a case in which it has not been traceable to some form of cerebral excitement with succeeding depression. Grief, imposed by the deaths of relatives and friends ; shock from failures of enterprises in business ; disappointments ; violent outbursts of passion ; remorse ; degradation ; and, most fruitful cause of all in this madly striving age, over-work of brain—these are the outside influences leading to the systemic change on which the phenomenon of intermittency of the pulse most frequently depends.—*Medical Times and Gazette, Jan.* 4, 1868, *p.* 3.

DISEASES OF THE ORGANS OF RESPIRATION.

19.—ON THE NATURE AND TREATMENT OF PULMONARY CONSUMPTION.

By Dr. C. J. B. Williams, F.R.S., Consulting Physician to the Hospital for Consumption at Brompton.

Pulmonary Consumption may be defined to be *a disease characterised by the consolidation of more or less of the lungs, with a tendency to degeneration, softening, and excavation of the parts affected, and this is attended with wasting of the whole body, and other evidences of its imperfect nutrition.*

In a large proportion of cases the consolidations, which precede the destructive process, occur in the form of *tubercles*, miliary or clustered ; not necessarily preceded by inflammation

in the lungs or air-passages. In others the consolidation is more uniform or diffused, and is the result of some form of inflammation of the pulmonary textures. In either case the progress of the destructive disease is marked by the appearance of yellowish opaque spots, which spreading, convert the consolidations, granular and diffused, into crude yellow tubercle, whilst this further tends to softening, disintegration, and final excavation of the affected textures.

It is this tendency to degeneration and destruction which stamps the *consuming* character of the disease, and the more strongly this tendency is manifested the more irresistible and rapid will it be in its fatal course. In certain cases the disease is so acute and extensive as to carry off the patient in a few weeks or months. In others it is more limited and slow, and may not destroy life for five, ten, twenty, or more years. In the former cases medicine has little or no control over the disease; decay and death invade the frame so overwhelmingly that there is neither sufficient power in nature to resist them, nor time for art to aid that power. One of the most vital organs of the body becomes suddenly invaded by a disease changing its structure, obstructing its functions, and spreading through it the seeds of further decay, which not only in the organ itself, but by the blood, diffuses its destructive influence though the whole system.

Let us briefly sketch the two most terrible forms of the disease.

A man of middle age is attacked with fever, with pungent heat of the body, cough, viscid expectoration, extreme oppression, and overwhelming weakness, resembling that of continued fever; and the likeness sometimes appears also in the coated or dry brown tongue, sordes on the teeth, and occasional delirium. The vesicular breath sound is superseded everywhere by bronchial rhonchi and mixed crepitation. On percussion the chest is dull nowhere, but less clear in the posterior than in the front part. This case might be supposed to be one of universal capillary bronchitis, with general pulmonary congestion. So it is; but this is not all. In spite of blisters and other remedies the breathing remains short and difficult; the pulse more rapid and feeble; the lips, cheeks, and nails become livid; clammy sweats break out, and the patient dies in the third or fourth week from his first attack. The lungs are found congested, and the bronchi loaded with viscid mucus; but more than this, innumerable miliary tubercles are scattered throughout the pulmonary tissue, and these are the obvious cause of the intractability of the case. They break out simultaneously, like the eruption of an exanthem, and by their numbers and bulk induce such an amount of obstruction and congestion in the

lungs as to destroy life before there is time for any considerable degeneration or softening to take place. This *acute tuberculosis* is the worst and most surely rapidly fatal form of consumption.

The second form of acute consumption begins with pneumonia in one or both lungs. The patient, generally a young subject, is of consumptive family, and may have previously had cough and occasional hæmoptysis. The fever attendant on the inflammation may not be very high at first, and the expectoration by no means so viscid and rusty, nor the crepitation so fine and even, as in simple inflammation of the lungs. But the symptoms are more persistent. The pulse and respiration remain frequent. The heat of the body, particularly of the chest, continues remarkably high, almost burning the ear of the auscultator as he examines the back. But this intense heat is alternated with occasional chills and profuse sweats at night. The cough continues distressing, and the expectoration becomes opaque, purulent, and clotty; the flesh wastes, and the strength ebbs away; and if the appetite does not return, the progress of consumption and decay is rapid. Auscultation reveals the steps of the destructive process in the lung. The affected part, or the whole side, or part of both sides, becomes dull on percussion, only varied with the cracked-pot note from the gurgling within; the loud tubular sounds are replaced by course crepitation, in parts amounting to gurgling; and the diffused bronchophony is modified into detached islands of voice, loud and protoriloquous, or into the snuffling or whispering sounds equally characteristic of a cavity. This form of *galloping consumption* may also prove fatal in a few weeks; and the lungs are found after death in a state of consolidation little more dense than the hepatisation of pneumonia, but their red is mottled with grey and yellow patches of tuberculous or aplastic matter, and excavated in various parts into numerous small cavities communicating with the bronchial tubes, and containing more or less of the same compound matter which was expectorated during life, consisting of mucus, pus, degenerating epithelium and exudation-matter, with disintegrated fragments of lung-tissue. This form of acute phthisis, although generally rapidly fatal, is not universally so. When not too extensive, it may sometimes be arrested and brought to a chronic state; and the chance of this result will very much depend on the recovery of the appetite, and the power of the stomach to bear strong nutriment, tonics, and above all, cod-liver oil.

And in a large majority of the cases of consumption the destructive element is still less extensive and less active, and its progress is much more slow; and we have both time and means to resist its inroads and to fortify the system against its opera-

tion to a greater or less extent. In the greater number of instances the disease begins with the symptoms of common cold, often referred to the throat as much to the chest; and there is, in truth, more or less of bronchial irritation and inflammation attendant on the development of the disease, and recurring with renewed intensity at the time of its increased activity. Often the disorder is mistaken for a common cold, until either its remarkable persistence, or the occurrence of hæmoptysis, of night sweats, of loss of flesh, or of some other uncommon symptom, gives intimation of its more serious nature. Then it is found that, in addition to the signs of bronchial catarrh, there are some of the signs of consolidation of the lungs, generally near an apex; slight dulness or raised tone of the stroke-sound at or below a clavicle, or at or above a scapula; a tubular character in the breath-sound and voice; an undue intensity or duration of the sounds of expiration, or a weakness or absolute obstruction of the inspiration; and sometimes the various slight degrees of crepitus substituted for the proper breath-sound; and various other signs which it is unnecessary here to detail. These signs, however, are the indications of *incipient pulmonary consumption*—that is, of a disease which tends, sooner or later, to injure and destroy the structure of the lung, and to deteriorate and waste the flesh and blood of the whole body. And the progress of this work of injury and destruction is marked by signs of increased density and diminished motion of parts of the lungs; by more of the moist crepitus, from augmented humidity, in and around the consolidations; and eventually by signs of excavation at one or more points, which announce the removal of the diseased tissue.

The progress of this disease may vary infinitely in time and in extent. The more extensive the mischief, generally the more rapid will be its progress, which goes on in the worst cases, uninterrupted by any check or pause, attended by the distressing train of symptoms, harrassing cough, opaque clotty expectoration, increasing shortness of breath, burning fever alternated with profuse sweats and chills, rapid loss of flesh and strength and colour, sometimes diarrhœa, and aphthous mouth, terminating in death in a few months.

But in other cases, and these are by far the most common, the destructive lesions are less extensive, and their progress is more slow and intermittent, and often seems in great degree to depend on occasional attacks from cold or other external causes, in the absence of which the disease may be quiescent or stationary, and may not destroy life for years.

Powerless as medicine is in the overwhelming and rapid types of the disease, it has yet considerable influence over

these milder forms; and the succeeding paper will give some evidence that under careful treatment life may be prolonged for many years in comfort and usefulness, and in not very few cases the disease is so permanently arrested that it may be called cured.

[Although an admiring pupil of Laennec as far as regarded his sagacity and accuracy in detecting and describing disease, Dr. Williams has never considered his views of the nature of tubercle scientific and satisfactory. From 1828 until now Dr. Williams has held and taught that tubercle is a product of degraded nutrition, a material defective in organisation, and tending to still further deterioration and decay. The progress of pathological research, so far from superseding this view, has in most parts remarkably confirmed and substantiated it.]

There is a common variety of *cacoplastic* deposit, which takes place when textural nutrition is degraded, still further than in false membranes and healthy cicatrices: this is the transparent, miliary, grey, and tough yellow forms of tubercle. Instead of (with Laennec) classing tubercle under the vague term of "accidental productions," or (with Carswell) as a "secretion *sui generis*," I have for many years been induced to refer it to a degraded condition of the nutritive material from which old textures are renewed and new ones formed, and to hold that in its origin it differs from the normal plasma, or coagulable lymph, not in *kind*, but in *degree* of vitality and capacity of organisation. Every gradation may be found between euplastic and aplastic deposits, the cells and fibres, which are the representatives of organisation, diminishing in number and completeness, and the material becoming more granular, amorphous, or more abounding in fat-globules, in proportion as the deposit is degraded; until, in opaque, crude, or yellow tubercle, it is altogether aplastic, consisting of an aggregation of granules and fat-globules interspersed with the mere traces or remains of cells.

I consider that the more solid forms of tubercle are entitled to rank among cacoplastic deposits, because, although destitute of vascularity, they seem to possess a kind of structure, like that of the lower kinds of granular deposit. In the semi-transparent grey, tough yellow, and miliary varieties, vestiges of cells and fibres are commonly seen. The affinity of these with granular degeneration and fibrous deposits is shown by their commonly occurring in the same subjects, and by their frequently exhibiting the same tendency to contraction. In a very large majority of cases of chronic granular disease of the kidneys or liver, there are found more or less traces of tubercle in the lungs, the chief seat of its deposition; and in very few

instances of chronic phthisis have I failed to find some degree of granular disease in the liver or kidneys. In acute phthisis and in acute granular disease local causes accelerate the degenerative change to a destructive extent in one organ before there is time for others to become affected.

We now proceed to trace the history of cacoplastic deposits by a few examples. The dense false membranes formed on the surface of serous membranes may be often seen to be surrounded with a radiated wrinkling or puckering of the adjoining parts, indicating that the abnormal substance has shrunk in size subsequently to its being first laid down. A similar contraction is noticed in the deposits that occur on and under the lining membrane of the heart and its valves, and here it causes a serious disturbance of the mechanism. The narrowing of the chest in some cases of pleurisy is in part dependent on the same property of cacoplastic deposits. This tendency of false membranes to contract was long since pointed out by Laennec and Hodgkin, and the fact was subsequently applied by Carswell to explain the diminished size of the liver in cirrhosis, which he considered to be due to a deposition in the intravascular and filamentous texture prolonged from the capsule of Glisson. I do not myself think that the contractile deposit in cirrhosis is confined exclusively to one texture, but believe that it is generally exuded from distended blood-vessels. False membranes which exhibit the same contractile character are sometimes found on the free surface of serous membranes, and especially along the course of the vessels, veins as well as arteries ; and depressions caused by atrophy of the substance of the organ beneath these deposits are sometimes seen in the liver and lung. There can be little doubt that these deposits originate in a fibrinous material exuded from the vessels in certain pathological states (congestion, chronic inflammation, and malnutrition), and forming a dense structure of low vitality, which tends by its subsequent contraction to constrict and compress the adjacent parts, and so more or less to interfere with the passage of blood through them, and consequently to derange their nutritive and vital operations. The same tendency is evinced in the higher (less aplastic) forms of tuberculous disease. Miliary or granular tubercles in the lungs, when in considerable numbers and not soon softening, cause a contraction in the substance of the organ, chiefly at the upper part, and a corresponding collapse in this portion of the chest. I have met with many cases in which a sinking of the infraclavian region has taken place before any signs of softening have occurred ; indeed, this is a very common sign of tubercles which remain long stationary. The still greater amount of pectoral collapse in the advanced stages of tuberculous lesions, although partly dependent on other causes, is also still in some

degree connected with the contraction of cacoplastic deposits in the lungs and pleura. In the peritoneum agglutinated tubercles often cause considerable contraction. I have seen the omentum puckered up by them into a knotty mass.

The microscopic examination of cacoplastic deposits, at different stages of their formation, throws considerable light upon the true nature of this process. When recent the effused matter consists of fibres, with a great quantity of granular, and more or less of amorphous substance, with or without an admixture of cells. At a later period, and after it has contracted, it is much more tough, and it is then found to be more distinctly fibrous, much of the granular and amorphous matter having disappeared. Old contractile adhesions and cicatrices are still denser, and when examined by the microscope are seen to consist of closely interwoven fibres, differing from those of fibro-cartilaginous tissue in being less regular and distinct. Grey or granular tubercles wither, and become more tough or horny ("cornify," Rokitansky), and thus remain quiescent. It appears, therefore, that it is by the partial absorption of the granular and amorphous portion of the deposit, and by the closer approximation of the fibrous or more organised constituent, that the condensation takes place. Hence it is that the structure thus contracted is less liable subsequently to the further degeneration to which cacoplastic products are prone. Certain it is, that of different portions of cacoplastic deposit in the lungs and elsewhere, those that are contracted remain unchanged, whilst the rest pass into the aplastic state of opaque and softened tubercle. The contractile process seems, therefore, to raise the deposit to a higher standard, so that, although still inferior in vitality to the adjoining textures, it gets to be tolerated by them, so to speak. But this very change may seriously injure the textures of organs by contracting and compressing their vessels, and interfering with their nutrition and other functions; and in this respect it may be compared with the highest, or most animalised variety of degeneration, the fibrous, which has already been described. It is in this way that the contractile diseases of the liver and kidneys gradually infringe on the circulation and secretion of those organs, and eventually prove fatal. The contraction which takes place near the apices and roots of the lungs in the more limited, and therefore more chronic, varieties of pulmonary tuberculous disease, often lays the foundation of emphysema of the organ and habitual asthma.

Having described the less degraded and degenerating form of cacoplastic deposits, it now remains to notice the variety which is of a lower character, and which tends to become aplastic. This includes all the commoner forms of tubercle. In the granular, miliary, grey, or drap-coloured tubercles of serous membranes

and of the parenchyma of organs there is found a dense homogeneous solid, closely resembling some of the cacoplastic deposits which have just been described. The resemblance, even the identity, of these may often be very well traced out in chronic or subacute arachnitis, peritonitis, and pleuritis; portions of the affected membrane are covered with diffused masses of semi-opaque deposit, which no one would hesitate to call false membrane, whilst in other parts separate granules of precisely the same deposit exhibit all the characters of granular or miliary tubercle. In chronic and scrofulous inflammation the matter effused is, from the first, less ductile and more consistent, and the granular condition consequently more generally prevails, and remains even in spite of pressure and friction; and it is admitted that the product of chronic inflammation of the peritoneum and of the arachnoid membrane is always more or less granular. In acute inflammaiion in tuberculous subjects recent lymph is commonly studded with opaque granules, which subsequently assume the character of tubercles.

But tubercular deposit is not always preceded by inflammation. In many cases tubercles are found so extensively disseminated through different textures, after few or no symptoms of inflammation, that it is quite impossible to regard them otherwise than as the result of modified textural nutrition.

Miliary tubercles exhibit a considerable variety in size and consistence, according to their age. When recently formed they are plump, and although feeling harder than the surrounding textures, yet they can be crushed with firm pressure between the fingers. But in lapse of time they contract in size, and become tougher, so that no pressure can crush them. This change in the lung is accompanied by a darkening of colour, so that old tubercles are often quite black. This is an indication of the tubercle having become quiescent or obsolete, and having little tendency to degenerate to the yellow state.

Tubercles rarely occur in numbers, or spread much, without exhibiting another change of appearance: they lose their semi-transparency, and become of an opaque or dead pale-yellow hue, like the colour of raw potato or parsnip. This marks the transformation to crude yellow tubercle first described by Laennec, and this is the result of a further degradation or degeneration of the deposit. The few fibres and cells which are to be detected in grey tubercle are no longer distinguishable here; the interstitial hyaline or amorphous solid is greatly diminished, oil-globules appear in its stead, and the mass becomes proportionately less coherent and more granular, and is now, indeed, quite *aplastic*. Generally the change begins in the centre of the mass, apparently because, there being no permeating vessels, the centre is the part furthest removed from the

vivifying influence of the blood. The cornea and other non-vascular textures occasionally become opaque in a similar way, in consequence of deficient supply of sustaining nutriment in the plasma, and may, indeed, entirely lose their organisation. A parallel form of degradation is that observed in the degeneration of fibrous and cellular tissues into granular matter, recently pointed out as resulting from malnutrition. But not only will deficient supply of healthy plasma promote the transformation of tubercle from grey to opaque ; an undue flow or accumulation of blood, produced by inflammation or congestion in the neighbourhood, will also accelerate the change, just as the same occurrence hastens the disintegration of lymph and of compressed textures in suppuration, the conversion of the plastic into aplastic, through the increased warmth and afflux of fluids exalting chemical affinities in a material which has none of that vital power of resistence by which living tissues maintain their integrity.—*Lancet, March* 21 *and* 28, 1868, *pp.* 369, 403.

20.—WHAT IS TUBERCLE?

At a meeting of the Clinical Society last Friday, an interesting discussion took place on a case of Fibroid Phthisis. Of late years, this name has been given to a disease which has long been recognised; but which, although differing in its duration and etiology from ordinary phthisis, has been included under the class of tuberculous affections; and the question at issue seemed to turn on whether this disease was of the nature of tubercle, or whether it should be described as a distinct variety of phthisis.

The word "tubercle" has been applied to so many morbid changes which differ from each other in their clinical history, as well as in their histological characters, that, from originally denoting a small rounded body, it has been made to include any process which results in the breaking down of lung-tissue and the formation of cavities. When a word thus becomes altered in meaning—when some use it only in a restricted sense, and others as almost synonymous with phthisis—it must follow that confusion arises in discussion; and this, while pathologists are describing the same morbid process, they will look upon it as tuberculous or not, according to their preconceived notions on the subject.

The term "tubercle" is beginning now to be restricted to the grey granulation: a definite body, formed of oval or round nuclei, and generally surrounding a vessel or bronchus; in cases of tubercular meningitis, it may readily be studied in the pia mater, and it is also to be seen scattered through both lungs or in other parts of the body; in yet other cases where the course

of the disease is not so rapid, the granulations may be found to be aggregated together, and in the most advanced portions to have become softened in the centre and formed cavities; in other parts of the lungs, grey tubercle may be seen scattered about and in various stages of transformation. Accompanying this pathological state, there is a distinct clinical history, which much resembles typhoid fever, and, like that disease, has great daily variations of temperature.

To the yellow or cheesy masses so often found in the lungs, Dr. Clark does not assign the name "tubercle." Under the microscope, they are seen to consist of round cells, with nuclei and granular contents, and, as they extend, nutrition being cut off from the central parts, they soften and break down. The origin of this "cheesy" matter may be a pneumonic product which has not been absorbed or cleared up, or a syphilitic or cancerous deposit, or as a result of embolism, or concretions in the bronchial tubes. This appearance, according to Dr. Clark, although to be found accompanying the grey granulations, may be met with independently of them.

In another class of cases, a more chronic change occurs, and, instead of a pneumonic product, a fibroid material results; the pleura is found much thickened, and proceeding inwards are numerous septa, which divide the lung into segments, and this change takes place in the perivascular or peribronchial tissue; it consists either in a proliferation of the connective tissue cells, or in an exudation of an amorphous material into the air-vesicles themselves.

This process is one which may last many years, and, although cavities may form, they extend but slowly, as there is such a dense wall of fibroid induration around. Generally affecting one lung, and the left more frequently than the right, it is often accompanied by an amyloid state of liver or kidney; it is also to be met with in rheumatic or syphilitic individuals or in drunkards.

Clinically, it is remarkable for its long duration, as the disease may last many years; the absence of night sweats and of any rise in temperature. It may be accompanied by hæmoptysis, diarrhœa, and loss of flesh; and physical signs of cavities or consolidation of lung-tissue are to be met with. Although it may occur without any grey granulations being present, yet this is not always the case; and, if this fibroid induration surround tubercles, it may be looked upon as due to a slow process of irritation, which, beginning in congestion, ends in a proliferation of connective tissue nuclei; while, in other cases, where the congestion is more intense or the change more acute, a local pneumonia results, and the cheesy material is met with.

By thus restricting the term "tubercle" to a definite histo-

logical product accompanying a well defined condition of body, and by giving a fresh name to processes which are different in their origin and course, Dr. Clark has thrown much light on a subject which has hitherto been very obscure. While the term "phthisis" may be kept as indicating a state the chief symptom of which is a wasting away, it will be well if we recognise various forms of it, as different from each other as the contracted gouty kidney from a large white one; then phthisis will be as genuine a name as Bright's disease, and the word "tubercle" will have a distinct annotation, and not, as heretofore, include a number of perfectly distinct pathological processes.—*British Medical Journal, March* 7, 1868, *p.* 222.

21.—ON SOME ANOMALOUS CASES OF ACUTE PHTHISIS.

By Dr. Sydney Ringer, Physician to University College Hospital.

[Certain anomalous cases, not following the same rules as the ordinary cases of phthisis, struck Dr. Ringer whilst investigating the temperature in that disease.]

In all the cases of which we shall first speak, the patients suffered from acute phthisis. The deposit was yellow tubercle, or, as it is now called, scrofulous pneumonia of the lung. This was either in the infiltrated or discrete form. Thus far these cases are in no way peculiar: their peculiarity consists in this—the deposit occurred first at the base of one or both lungs, and was always most abundant there, and was often almost entirely absent on the upper lobes, even so when the lower lobe was absolutely solid, or riddled with small abscesses, or converted into one large hollow.

In their course, they simulate, as regards their physical signs, various other diseases. The difficulty in the diagnosis is still further increased by the generally accepted doctrine that tubercle, whether grey or yellow (*i.e.*, scrofulous pneumonia), always invades first and most the upper part of the lungs. By the aid of the temperature, with the course the physical signs and symptoms run, we have been able to obtain a correct conclusion of the nature of the disease—a conclusion afterwards confirmed by the post-mortem examination.

We shall at first confine our attention to the physical signs met with, at different periods, in these cases. Some of the patients were first seen by us when the disease had well progressed; others were under our care from the very commencement, and were carefully watched throughout.

At one time of the disease the only physical sign present may be crepitation at either base, which often has all the characters

of pneumonic crepitation in perfection ; at other times it is a little larger ; but, as every practical man knows, in pneumonia itself a very great difference is met with in the size of the crepitation.

At this stage it might be easily thought that the disease was genuine pneumonia. This error is the more probable, as the disease may begin acutely with high fever, stitch in the side, slight cough, and scanty tenacious expectoration, and this crepitation may be from the first present, and detected by auscultation. But even at this stage our suspicions will be aroused by various circumstances. Thus, generally there is but little weakness ; that extreme prostration so common and characteristic in pneumonia is absent. The patient walks with tolerable ease into the room or moves without assistance in bed. Another circumstance which, in conjunction with others to be mentioned, will assist us in the solution of a difficulty similar to that under our consideration, is that the breathing is but little oppressed, and would not strike an observer as being in any way affected. It is, however, generally a little hurried. The expectoration is not rusty unless there has been decided hæmoptysis ; but when such occurs, and for a few days after it has ceased, the expectoration may be rusty. The very fact of the hæmoptysis which preceded the rusty sputa would excite our apprehension, ard would lead us to fear that the patient was consumptive. Yet with all these characters, apart from hæmoptysis, a positive conclusion cannot be formed. In a few days, however, the course the physical signs observes clears up all our difficulties, and confirms the suspicions of the case, and justifies the diagnosis of consumption. For while in simple pneumonia the few physical signs mentioned above are soon accompanied by some dulness on percussion, with alteration of the breath sound, and in a few more hours by absolute dulness with great increase of the vocal fremitus and tubular breathing, such is not the case with scrofulous pneumonia of one base. But instead of this, day after day passes, and yet the physical signs remain just as few and indeterminate ; the crepitation continues, or even this may be heard abundantly one day but be unheard on the next, and again on the following one be as abundant as ever ; sometimes a little dulness may be added to the crepitation, but this is usually slight. This almost stationary condition of the physical signs is always suspicious of scrofulous pneumonia, and the longer it continues the more characteristic does it become of the disease.

In some cases the paucity of the physical signs may continue for weeks with only a small amount of dulness added, and without any appreciable alteration in the characters of the breath sound. This long continuance of such physical signs is of

itself quite sufficient to prove the disease before us is not simple pneumonia, and, added to the condition of the temperature, is amply sufficient in most, if not in all, cases to conduct us to a correct diagnosis.

Of œdema of the lung, which may be accompanied by fine crepitation at the base, it is hardly necessary to speak, as the general condition of the patient will generally decide whether such a state of lung is present or not.

Then, again, pulmonary apoplexy, which is often seated at the base of the lungs, may be accompanied by crepitation like pneumonic crepitation. But then, again, the presence or absence of a disease sufficient to cause pulmonary apoplexy, with a general consideration of the symptoms and physical signs, will settle any doubts in our minds in such a case; and, again, in these two last-named diseases the temperature is natural, as far as a limited experience in this matter will allow me to speak.

In what way does the temperature comport itself in scrofulous pneumonia? Some of its characters are of greater importance than others. We shall speak first of those of lesser importance. The amount of elevation in scrofulous pneumonia is usually less than in simple pneumonia, generally not reaching higher than to 102° and 103° Fahr., while the temperature of simple pneumonia is often 104° and 105° Fahr.; yet this distinction is by no means a constant and hence important one. Another difference in the temperature of the two diseases is this: while in scrofulous pneumonia it often falls much in the morning, so that the diurnal variations are great, in simple pneumonia the elevation is maintained at much the same height throughout the day; but, again, too much stress must not be attached to this character of the temperature. The great point to look to is the time it remains elevated. In simple pneumonia the inflammation ceases at some time between the fifth and tenth day, and with this cessation the temperature falls and becomes natural. Should the temperature remain high for a longer period than this, then we are justified in the diagnosis of scrofulous pneumonia, if the physical signs given above are present. Thus, this combination of circumstances—namely, crepitation, fine or coarse, heard at the base, with an elevation lasting twelve or more days—is sufficient to lead to a correct diagnosis of scrofulous pneumonia, and every additional day of continuance of these circumstances confirms the conclusion.

We have said if crepitation be present; but it is not necessary to have the crepitation exactly like that often met with in pneumonia. Indeed, in most cases such a rhonchus is not present, but very generally one with many of its characters. Generally,

in scrofulous pneumonia, it is a little larger, and often accompanies expiration as well as inspiration.

From this point the disease gradually increases, and with this increase the physical signs and symptoms alter, until another phase of the disease is presented—such a one as may come first under the notice of the Medical man—and then the diagnosis will have to be made on the strength of the signs of this new phase. This new condition of things thus comes about. After some days, or it may be weeks, dulness at the base occurs. This increases very slowly in extent and in intensity. At the same time, and *pari passu* with this increase of the dulness, the vocal fremitus becomes more marked, and the breathing blowing, metallic, and soon perfectly tubular.

Here, again, are circumstances sufficient at first sight to lead to the impression that the disease might be simple pneumonia of the second stage, as we have dulness at the base of one or both lungs, with increase of the vocal fremitus, with tubular breathing and fine true crepitation. Yet there are peculiarities in these characters which suffice often for a determination of the nature of the disease; for in most of these cases the crepitation is peculiar in this respect. As the physical signs deepen in intensity, this fine rhonchus continues, and is heard over the whole of the consolidated lung; on some days being small in quantity, or even absent for a short time, and replaced by a larger rhonchus, but soon it again returns, and even when the physical signs have reached the intensity described above, this crepitation is heard over even the most dull parts. If we had watched a case thus slowly progressing, the task in diagnosis before us would be comparatively easy; but the case is otherwise when we are summoned to see a person for the first time with signs such as those described. The fact of the crepitation being heard over the whole of the consolidated part of the lung and not at its confines, its seat in simple pneumonia, would be a circumstance that should attract at once the attention of the observer and excite his suspicion of some other disease than simple pneumonia. Then, in such a case as we are now supposing, and I have in each instance quoted from notes taken at the bedside, the previous history of the patient would assist us very greatly, as, if the patient had been ill for some weeks or months, and we could connect his then physical signs with his previous illness, we should be convinced that simple pneumonia was not the disease he suffered from. But it is not always easy to decide such a question; and it may be he has suffered with some complaint which has been complicated with simple pneumonia. If such a doubt arise, a few days' observation will set the difficulty at rest, for the temperature will come to our assistance. On examination,

if the case be one of scrofulous pneumonia, we find this to be 102 or 103 every evening, and to have all the characters witnessed in the first stage of the disease. Such an elevation, or one higher, would occur in simple pneumonia, but if the consolidation were due to the last-named disease, then we should expect the inflammation, and with it the temperature, to soon decline, and the last to become natural, as, if a stage of consolidation were reached, the disease must have lasted a little while and thus be near its termination. Now, if we find the temperature continue elevated day after day, we must conclude, with such physical signs, our patient is the subject of scrofulous pneumonia.

When this phase of the disease is reached, the physical signs characteristic of it continue week after week, even month to month, with scarcely any alteration, or with merely a very slow increase in their intensity and extent. The continuance for some time at the base of great dulness, increased vocal resonance, tubular breathing, and wide-spread crepitation, with an elevation of the temperature to 102° and 103°, justifies a diagnosis of scrofulous pneumonia. The symptoms met with at this stage of the disease are not those of simple pneumonia; the weakness is less marked, and the breathing but little hurried, and the pulse respiration ratio much less disturbed. Often the patients can get about, and even do much of their work, while their breathing appears to be quite calm.

If life be continued long enough, the disease advances another step, and cavities are formed. Then to the tubular breathing, absolute dulness, diffused crepitation, there are added at places cavernous breathing and cavernous rhonchus, sometimes even amphoric note on percussion. This state of things may be present at the base while the upper part of the chest is free from unhealthy physical signs, and when, as the post-mortem shows, the upper part of the lung is nearly free from the deposition from the disease.

Here is a collection of physical signs which might suggest diagnosis of ulceration of the bronchial tubes at the base, leading to the production of cavities and to consolidation between the bronchi. Such a disease is occasionally met with. The latter may run a rapid or slow course. It is very generally a very chronic disease, and then does not produce any elevation of the temperature, or a very slight and very occasional one to 100° Fahr. This distinction between the two diseases was lately well shown in our wards, where we had a patient suffering from scrofulous pneumonia, and opposite to him another with this ulceration of the bronchial tubes. The diagnosis in both cases was confirmed by the post-mortem examination. In the case of scrofulous pneumonia, the temperature was very considerably

elevated every day; while with the patient who suffered from ulceration of the bronchial tubes and the formation of cavities the temperature was almost always natural, and only occasionally rose to 100° Fahr. There are also usually marked differences in the physical signs of the two diseases, while also the expectoration in ulceration of the bronchial tubes of that kind of which we are speaking is generally different from that of scrofulous pneumonia.

Most of the cases on which our remarks are based were examined after death, and the state of the lungs carefully ascertained. It was then found that the lower part of the lung, often the whole lower lobe, with perhaps part of the upper, was completely consolidated, and presented the appearance of what was formerly called infiltrated yellow tubercle, but which has now received the name of scrofulous pneumonia. The whole or the lower part of the lower lobe was absolutely solid, or here and there were seen a few small spots of less affected or healthy lung. The consolidated tissue was quite airless; the section dry, of a straw or grey colour, mottled with spots and streaks of black pigment. It broke down with some ease, and the fracture was granular or cheesy. In some parts it was softer, more brittle, than at others, and had more the characters of ordinary pneumonic infiltration. These parts, the most recently affected, were more raised above the cut surface than the older deposits, and also were more granular, less opaque, and, as we have said, more brittle and more easily broken down. The older parts gave what has been termed the horse chesnut look and consistence.

The disease appeared to originate in two ways in a manner similar to that when the disease first affects the upper part of the lung. Sometimes small nodules, the size of pins' heads or peas, were formed, and by their increase in number the lung was gradually destroyed until, by their coalescence, large tracts of lung were consolidated. In other cases there were seen innumerable little straw-coloured spots, closely placed together, and looking as if isolated vesicles were filled with the diseased products. Each of these was separately affected, and those in their immediate neighbourhood were healthy to look at. These latter in their turn appeared to become affected, and thus extensive consolidation is attained. Where the consolidation is great and the deposit of some duration, it is tougher, smoother, denser, less granular, and more opaque than at those places where the deposit is of recent formation. These consolidated portions were sometimes riddled with small cavities formed apparently by the deliquescence of the diseased lung, for the cavities are then everywhere immediately bounded by consolidated lung, and not by a fibrous capsule. These cavities at the

base may reach to the size of a large orange. The consolidation may be thus intense, and the excavation thus advanced, while the upper lobe may be almost free from the disease, and contain only a few scattered nodules of yellow tubercle. Sometimes the disease begins at one base, and then gradually invades the whole of that lung, and then attacks the other lung, setting first upon its apex, and involving this most extensively in the disease. At other times both bases are the first to be affected, and the disease is always most marked at these parts.

In these cases, it appears, we have yellow tubercle (scrofulous pneumonia) affecting primarily the base. Yet in these cases the physical signs and the morbid anatomy are somewhat different than when the disease is situated at the apex. Thus, the crepitation, its persistence and wide-spread character, so marked when the disease is at the base, is rarely met with in attacks which commence at the apex. The tubular breathing, again, which is so intense in the form of the disease of which we are writing, is absent where the disease fixes on the apex. This latter difference is well explained by the anatomy of the two forms. The consolidation is greater—that is, it is more general and more intense—where the base is primarily affected, and passes less slowly into excavation, than is the case with tubercle at the apex. In respect of the symptoms, they are the same whether the disease first seizes upon the apex or the base. Copious spitting of blood may be one of the earliest symptoms, perhaps the first to attract the patient's attention and to excite his fears. This may be often repeated. Where it occurs, the expectoration, for some days after it has ceased, may be rusty in colour, and this appearance of the phlegm may easily lend countenance to the idea of simple pneumonia being present. The cough is, very generally, very tearing and hacking, frequent, and sometimes paroxysmal. It is one of the most distressing of the symptoms, causing from its violence and hardness much pain to the patient. The expectoration is at first composed of a mucilaginous-looking fluid, a little opaque, rather tenacious, very little aerated, and looks not unlike rice-water, but is thicker. It is at first free from purulent-looking matter. As the disease increases, the mucus becomes mixed with yellow purulent-looking expectoration, which may be pretty equally mixed with the mucus, or take on the shape of pellets. After a time, mucus ceases to be present, and all that is brought up is purulent. These are the characters of the expectoration in the various stages of ordinary phthisis.

The course of the disease is somewhat variable; as with the disease when situated at the apex, its course is for the most part a downward one, and the disease almost always ends in death. The temperature marks well these variations in the severity of

the disease. In some cases throughout it is elevated, every day rising to 103° or 104° Fahr., while in the morning it often falls very considerably, even to the degree of health. In other cases, after an elevation of some weeks, a gradual fall occurs, and continues till the temperature of health is obtained, and continues at this for a very long time, with an occasional rise to 100° or 101° Fahr. After a course of this kind, it may again rise considerably, and remain elevated till the patient dies. In more favourable and in exceptional cases the temperature becomes natural, and remains so while the patient recovers strength. His symptoms disappear one by one till good health is restored. With each temporary lull in the course of the temperature there is generally witnessed a decided improvement in the condition of the patient. This is sometimes so marked as to raise hopes of a permanent recovery—hopes which our experience shows us to be in most cases deceptive.

Such is the course the temperature adopts in all forms of acute phthisis, and, in conclusion, I may state that a very extended experience enables me to affirm a statement put forward by me in a pamphlet some time ago—namely, that a disease of the lung causing a considerable elevation of temperature lasting many days—say ten to twenty, or more—is almost always due to the deposition of either grey or yellow tubercle. But one exception to this general statement has as yet occurred to me, and there the elevation was due to acute inflammatory ulceration of bronchial tubes at the base of both lungs, leading to rapid excavation of the organ.—*Medical Times and Gazette, Feb.* 29, 1868, *p.* 226.

22.—PHTHISIS PULMONALIS AND ITS TREATMENT BY "PANCREATIC EMULSION OF FAT."

By R. J. Kinkead, Esq., A.B., &c.

In order to treat phthisis pulmonalis with any hope of being successful, it is absolutely necessary to consider carefully as much as we know of the pathology of the disease, the exciting and predisposing causes, and to inquire into the nature of what we call the tuberculous deposit.

It is only by so doing that we can act from knowledge like men endowed with reason, and not by guess-work and routine practice, in many instances sacrificing our patient's lives, and bringing disgrace on our profession.

It is this knowledge which enables us, from amongst the multitude of remedies offered for our use, to choose those likely to prove beneficial, and to reject the useless and injurious. This knowledge enables us also to avoid the too common mistake

of thinking that a medicine which in some cases does good, must be a specific, and of itself and by itself, able to cure phthisis, and so neglect that combination of remedies which, in this disease, is most essential to guide it to a successful issue.

Our knowledge of the constitution of tubercle is anything but satisfactory. Two opinions are chiefly supported by pathologists, one class affirming that tubercle is an exudation essentially morbid, the other, that it is merely a retrograde metamorphosis of pre-existing structures, either natural or morbid.

One of its essential characters is that, however deposited, it is incapable of development, but on the contrary, has a constant tendency to degenerate. Tubercles are either grey or yellow; the grey are tough, soft, of a pearl grey colour, have a certain degree of organisation, being composed of round, oval, or triangular-shaped bodies imbedded in a basis substance, and are soluble in liquor potassæ.

The yellow appear to be a degenerated, and more especially a fatty degenerated, form of the grey.

Tubercle is generally produced without inflammation, as implying pain, redness, swelling, and the production of lymph or pus. The question now arises as to the origin of tubercle. What are its exciting and predisposing causes? The exciting causes are, generally speaking, cold, and breathing an atmosphere loaded with irritating particles.

Our knowledge of the predisposing causes is not at all so clear. What we do know seems to be something like the following:— That it is an essentially constitutional disease. Whether local irritation, such as living in an atmosphere loaded with minute atoms of organic matter, can occasion the local development of tubercle without the constitutional tendency being present; whether "the knife-grinder's rot," the so-called cases of "consumption" in flax-scutchers, &c., are really cases of tubercular phthisis, or merely of hectic fever, induced by numerous spots of circumscribed inflammation, running on into abscess, or causing excessive purulent expectoration from inflamed bronchi, occasioned by the lodgment of minute particles of extraneous matters in the lungs, is yet to be solved. But be this as it may, the constitutional nature of tubercle, whether developed in the head, lungs, or mesentery, is one of the best-established facts in medicine. At the head of these constitutional predisposing causes stands hereditary taint, and then comes whatever tends to bring the system into a state of deficient nutrition.

In this state of the system, in which tubercle is likely to be developed, we cannot say that the blood is in fault so much as the pabulum, which ministers to the nutrition of the body, through the blood, which impoverishes the blood, and, as a consequence, brings the tissues of the body to an impoverished

condition likewise. In this state the blood becomes albuminous, and white corpuscles become increased.

The gastric and intestinal acids become greatly augmented, more than neutralising the alkaline secretions of the saliva and pancreatic juice, and so render them incapable of preparing fatty matters for absorption, and under certain circumstances render the albuminous constituents of the food more soluble. Hence more albuminous than oleaginous elements enter the blood, and cause the albuminous character of the blood, which, infiltrating the tissues, may in some instances be changed by coagulation into grey tubercle.

It is a certain fact that in most cases a low state of the system, accompanied by a poor state of the blood, is, if not a certain, at least a most probable predisposing cause, and in all cases tubercular phthisis is accompanied by this cachectic state of the body.

In considering the progress of tubercle, the tendency to degenerate which characterises it is an example of what has not inaptly been called the "vis naturæ medicatrix."

Grey tubercle degenerate into yellow, and the yellow to a puriform mass, which sometimes excites just sufficient inflammation to cause its own expulsion, leaving a cavity behind which (if there are no more tubercles or cavities in the neighbourhood) contracts. The walls pucker in, and finally become obliterated, or the watery particles may be absorbed, the earthy constituents concrete, and the mass be converted into a hard chalky substance, principally consisting of phosphate and carbonate of lime, which is sometimes coughed up, or covered by a false membrane, may remain for years harmless in the lungs.

Cases of phthisis pulmonalis may, in my opinion, be divided into three classes :—

I. Cases in which tubercles are developed rapidly in great numbers, and of small size (miliary), occupying, perhaps, the greater portion of the lungs (tubercular infiltration of Baillie), accompanied by inflammation, and running a rapid course.

II. Cases in which tubercles are confined to one portion of the lungs, but are constant there, appearing for an uncertain time neither to get better or worse, but occasioning a certain amount of ill health—throat or stomach symptoms being most frequently complained of.

III. Cases in which the tubercles are deposited in crops in limited portions of the lungs, giving rise to febrile exacerbations, and intervals of health, according as the tubercle is deposited and got rid of by expectoration or concretion.

In the first class we can do little or nothing. In the second, and more especially in the third, class we can do much.

On being "called in" to a case of phthisis our efforts must be directed to subdue any inflammation present, to allay any irritation in the lungs which would encourage the growth of tubercle; secondly, we must endeavour to procure the elimination or absorption of the tubercular matter; and, thirdly, we must strive to prevent the redeposition of tubercles.

Our treatment of the inflammatory attacks must be guided by the symptoms and nature of each case. The remedies that would produce the best effects in one might be most injurious in another.

The treatment likely to produce the elimination of tubercles is almost identical with that necessary to prevent their redeposition, and constitutes the, so to speak, "specific" treatment of phthisis, and may, in a word, be said to consist of putting into and keeping the body in as perfect a state of health as possible.

I purposely omit to speak of the efficacy of climate, although I believe that in most cases a judiciously chosen climate is absolutely essential to a perfect and permanent recovery. The other indication will be best obtained by procuring the assimilation of fats which supply respiratory food to the lungs and help to enrich the blood.

At the head of oleaginous preparations stands cod-liver oil, of which it is not stating too much to say "that its use has, to some extent, removed tubercular consumption from the list of incurable diseases."

The chief obstacles to its administration are the disagreeable taste, and its occasioning sickness. The class-books teem with recipes to obviate the former; the latter, I believe, is occasioned, not by any intrinsic quality of the oil but by that excessive acidity of the gastric and intestinal secretions so characteristic of scrofulous cachexia, as to have gained the name of "strumous dyspepsia." This acidity may, in many instances, be overcome by combining alkalies with the oil. But even this method will fail in some cases, and the stomach still continue to reject the oil.

In these cases, and in those in which the repugnance to the taste cannot be overcome, I have found "pancreatic emulsion of fat" a most valuable agent. The office of the pancreatic juice in the body (it has alkaline properties) is to emulsify the fats and render them fit for assimilation by the lacteals. The acid state of the gastro-intestinal tract in phthisical patients neutralises the action of the pancreatic secretion, and in some cases causes an excess of acidity. Now, pancreatic emulsion offers to the stomach the most easily digested fat (mutton suet) prepared for absorption by the very juice which disease has neutralised. So that, in one case, we have pancreatic juice neutralised by acid, and pancreatic juice and fat free to act,

and, therefore, the fat easily absorbed; and where the acid is in excess by the addition of an alkali, we have pancreatic juice and alkali neutralising acid, and pancreatic juice and fat free to act. But what is perhaps its most important action, is the educating the stomach to receive and digest oils, for, after the emulsion has been administered for some time, it will be found that cod-liver oil will agree in those cases in which before it did not. —*Medical Press and Circular, Feb.* 26, 1868, *p.* 178.

23.—THE GRAPHIC METHOD APPLIED TO THE MOVEMENTS OF RESPIRATION.

By David C. M'Vail, Esq., House-Surgeon to the Alnwick Infirmary, Northumberland.

Somewhat more than six months ago, the idea occurred to me that the graphic method might, with very great advantage, be applied to the movements of respiration; and after considering the matter very attentively, I had a model constructed, which, on being tested, satisfied me that the idea would work beautifully, if properly carried out. Accordingly, I engaged a first-class workman to construct from the model and some drawings a small machine which I had every reason to believe would answer the purpose. And I have not been disappointed, for the instrument, so far as I have yet wrought with it, answers my most sanguine expectation. The "spirograph" (for so I purpose calling it) in no way resembles the sphygmograph, except in so far as both it and the sphygmograph resemble all the other graphic instruments—namely, in that they write on paper moved by clockwork. Nor is it from any love of novelty that it differs from its elder brother, but from the fact that no instrument constructed on the plan of the sphygmograph would be of the slightest use as a spirograph. And the reason of this is very obvious. As most of the readers of the Lancet are aware, Marey's improved instrument consists essentially of a lever of the third kind, there being a fulcrum at one end, and the pen which writes on the moving paper at the other, the power being applied between and close to the fulcrum, in order that the comparatively small motion of the pulse may be considerably multiplied. Now, of course, the pen moving at the end of the lever would, if a uniform motion were applied—such as would be given by a small sewing machine,—describe, not perfectly straight lines (as, to be theoretically correct, it ought to do), but a series of arcs of a circle, thereby necessarily confounding the curved tracings caused by the instrument itself, and those which may be imparted to it by the part under examination.

Now, in the case of the sphygmograph, where the motion of the pulse is so very small, and the circle described so very large, the imperfection is practically of but little consequence, although I can see no reason for working with an incorrect instrument (especially one so delicate) when a perfect one can be so easily constructed. But in the case of respiration, where usually the motion is pretty considerable, and in forced breathing is very great indeed, the lever is quite inadmissible; for any lever of a workable size would give tracings so very much curved as to be quite useless. For the markings to be of any use the lever would require to be several feet in length, and the pencil end would move through such a space, and, consequently, would require paper of such breadth and length as to need a moving power little short of a small steam-engine to work the paper drums. And, besides, it would take so much power to work the lever that its delicacy, and consequently its truthfulness, could not be relied on.

The instrument appears to me to possess the following recommendations :—

1st. It is exceedingly simple and portable, for although the one with which I am at present working is in size about as large as a small French clock, there is no necessity for its being much if at all larger than a fair-sized snuff-box.

2nd. It can be applied to any individual part of the chest or abdomen, and to any extent of the chest or abdomen at once that may be desired.

3rd. It is, if properly made, (the teeth especially must be properly cut,) *absolutely correct;* for if the piston be worked by a uniform motive power, the tracings must be quite rectilinear.

4th. By having variously shaped end-pieces for the piston it will, I am certain, be easily applied either to the parts concerned in respiration, to the heart, or to the blood-vessels; in fact, it will be a spiro-cardio-sphygmograph, or for brevity's sake, a "motograph." My present instrument is rather too large for the arteries, but there is not the slightest difficulty in the way of constructing one that will do for all.

As this paper is quite long enough already I must defer at present giving any account of my practical work with the spirograph, but will forward a brief account at an early date.—*Lancet, March* 7, 1868, *p.* 312.

24.—TREATMENT OF HOOPING-COUGH BY THE INHALATION OF CARBOLIC ACID.

By V. W. Blake, Esq., Birmingham.

Many of my patients suffering from hooping-cough having derived great benefit from the inhalation of carbolic acid, I

have designed a simple apparatus, manufactured by Mr. T. P. Salt, of Bull-street, Birmingham, for the evaporising and diffusing of the carbolic acid in the atmosphere of the rooms occupied by the patients.

The effect of its inhalation on hooping-cough is marked by its quickly reducing the frequency of the cough and cutting short the spasmodic paroxysm. I have some cases of phthisis pulmonalis under the same treatment, which, I have every reason to believe, are much benefited by its inhalation.

As a fumigator and a disinfecting agent in fevers, &c., I think the evaporiser will prove very useful, and I now invite my professional brethren to make trial of it. In hooping-cough its success is very great.—*Medical Times and Gazette, April* 11, 1868, *p.* 405.

DISEASES OF THE ORGANS OF DIGESTION.

25.—ON THE SUCCESSFUL USE OF ARSENIC IN CERTAIN CASES OF GASTRIC PAIN.

By Dr. Arthur Leared, Senior Physician to the Great Northern Hospital.

[Dr. Leared considers that neuralgia may attack the stomach and intestines in common with every organ in the body.]

This primary pain occurs by no means so frequently as that which attends dyspepsia. There is, perhaps, no kind of suffering more severe when in its worst form. The anguish of extreme pain is supplemented by the anguish of utter prostration, and what the patient calls a sinking sensation often adds to his distress. This sinking and other indefinable sensations in the stomach are probably due to its connexion with the solar plexus of the sympathetic nervous system.

The painful affection happily intermits; but the intervals between the attacks, which at first may have been measured by months, come at length to be spanned by weeks, or even by days, and, in some instances, the patient is attacked even on successive days. The seizure happens with but little warning, and generally when the stomach is empty. The patient may at the time be in his usual health, which, however, as will be afterwards seen, has been impaired by nervous exhaustion.

The pain is no less variable in degree than in kind. In some cases, it is intense and fixed to a spot under the epigastrium. Sometimes it is more diffused and cramp-like, as if the stomach was being forcibly dragged or compressed. The pain may even extend over a great part of the abdomen. When it has continued for some time, vomiting of a glairy fluid, or of bile, or

of food, if any be present, ensues. In the less severe attacks, there is no vomiting.

When the pain is intense, the state of the patient borders on collapse. The pulse is feeble; often slower than natural. The surface is bathed in cold perspiration. The face is pallid, and the features pinched. He rolls on his bed or on the floor in a manner which expresses great suffering. In some instances, stimulants or food give relief; but in others they increase the pain. The length of the attack, like the intervals between them, is very variable; it may only last half an hour, or extend to several hours. For some time after it has subsided, the epigastrium is sore on pressure. In other respects, with the exception of great prostration, the patient feels well. The tongue is pale and flabby, and the bowels may be quite regular. He eats with appetite, and digests quickly.

Most men in practice can call to mind cases to which the foregoing description applies, in which so-called gastric colic, or intense gastrodynia, has time after time unaccountably recurred. They will also bear me out in the assertion that ordinary sedative and tonic treatment is of little avail. But, if the pain is not connected with indigestion, can we be certain that the stomach, and not some adjoining part or organ, is the real seat of suffering? The position of the pain, the sinking and allied sensations, plainly indicate to the patient that it is so, and vomiting very often affords additional proof. As has been said already, the pain always precedes the vomiting, which is, therefore, caused most probably by reflex nervous action. In every instance of the disorder which has come under my notice, the subjects of it were persons of middle age, debilitated by some influence previously at work—such as loss of relations or friends by death, or that no less active cause of depression, loss of money. Malaria was also specially noted as being an excitant.

Dr. Budd has described an affection of the empty stomach "closely allied to waterbrash", brought on by similar causes, except that no mention is made of malaria. He cites two cases which would range under the description of the disease now given. But there is in general a wide distinction between it and waterbrash. Dr. Budd's chief remedy is hydrocyanic acid for the pain, and tonic medicines between the attacks. In "persons", he says, "who have once had it," the disorder is very apt to recur.

Dissatisfied with the effects of ordinary remedies, I thought of giving arsenic a trial. There can be no question of the extraordinary value of this drug in many forms of disease, and notably in some neuralgic affections of the head. If, then, it was a true conclusion that this gastric disorder was a neuralgia, the means by which it is subdued in the one situation ought to

be effective in subduing it in the other. Here is the first case in which the remedy was tried.

A lady, of stout habit, 46 years of age, consulted me in July 1863. The sudden death of her husband by accident, and the anxieties about a large family, weighed upon her heavily. About once a week she suffered such violent pain in the stomach, that, although it seldom lasted more than half an hour, left her in a weak condition for a long time afterwards. The pain would come on quite unexpectedly, whether the stomach was empty or contained food; and it was not influenced by diet. There was no habitual gastric symptom except flatulence, and the bowels were quite regular. Bismuth and various remedies were prescribed with little effect. Liquor arsenicalis was now given, at first in doses of three minims, subsequently increased to five minims. This treatment was only continued for a fortnight. The attacks were promptly suppressed by it; and, in a letter written to me at the end of this period, she says that she had already, in consequence of her improved health, encountered with impunity a great deal of fatigue, both mental and bodily. In the course of the following year, the disorder relapsed for a time, apparently from increased anxieties about her children, who had got fever. In this instance the treatment was not continued sufficiently long. Although the beneficial action of the remedy was very apparent, I quote it as one of my least satisfactory cases.

In the following case, the effect of treatment was very marked.

I was consulted, in January 1866, by a gentleman, 54 years of age, strongly built, but thin and worn looking. His habits were temperate. About two years previously, he was for the first time seized with a most violent cramp-like pain in the stomach, from which he afterwards suffered many repetitions. At first, the intervals between the attacks would be as long as a month; but for sometime before this he had been rarely free from them for a single day. They generally came on at night after going to bed; and it is worth noting that he dined early. The pain lasted from two to four hours; and he described it as being almost insupportable. It was attended by vomiting of acid fluid, which however did not relieve his suffering. But, with the exception of slight flatulency, he had none of the ordinary symptoms of indigestion. The appetite was good, and the bowels acted well. He had right inguinal hernia, and told me that the late Dr. Babington, under whose treatment he was for some time, was of opinion that the attacks depended on the hernia. A variety of treatment had been tried without effect. He was director of a public company, which, about the time of his first seizure, was broken up under unpleasant circumstances, and

caused him great trouble and anxiety. This was ascertained by questioning, and it helped the diagnosis materially. Liquor arsenicalis, in two mimim doses, was ordered to be taken after food three times daily. On the fourth day from commencing to take the medicine, he called and told me, with the exception of the first night, he was free from the attacks, and his nights were altogether better than for a long time previously. The dose was then increased to five mimims. On the eleventh day he again called. His nights had been good, and, except on two occasions in very modified degree, there had been no pain. Six mimims of the arsenic solution were now prescribed as the dose. On the eighteenth day, he reported himself as going on well; but there had been two slight attacks of pain. The dose was increased to seven mimims. On the twenty-fourth day, or six days from last report, he stated that there had been no return of pain. He was rapidly regaining the appearance of health. In order to make the cure more certain, the medicine was continued for two weeks more; the dose having been increased to eight mimims. I saw him lately; he has had no return whatever of the pain, and is in every respect well.

In March of the present year, a case was brought to me for consultation by Mr. Wheeler, of Bexley, under these circumstances. The patient was 38 years old, and rather robust. He had had, for three or four years previously, frequent eructations of what he called acid fatty fluid. But he brought up the same fluid in larger quantity about six p.m. daily, and this was afterwards repeated two or three times during the evening. Severe gastric pain preceded, and was relieved by these discharges. He was flatulent; but the appetite and digestion were good. The bowels were moved several times daily, and the fæces were frothy, and often mixed with mucus, described by the patient as closely resembling frog-spawn. Finding that he had had ague, although many years previously, and that the spleen was now large and tender on pressure, I advised a trial of quinine in three-grain doses, combined with bismuth three times daily; this was continued for some time with little result. I then suggested that arsenic should be given, the dose being gradually increased, and that counter-irritation should be kept up over the spleen. These measures were attended by cessation of the pain and gastric discharges, and great improvement in the condition of the bowels.

Although other remedies were here employed, there can be no doubt that arsenic was the effective agent. It is interesting to observe that, notwithstanding the irritable state of the bowels, it acted as a sedative. The case differs in several particulars from those which precede it. It was a kind of water-

brash, but differing essentially from that commonly seen, in which arsenic would act as a strong irritant.

The following case shows that the same treatment has proved useful when the pain in the stomach depended upon irritation transmitted from another organ.

The wife of a tradesman consulted me on account of the "dreadful agony" caused by taking any solid food. She had lived almost exclusively on milk for three months previously. There was great flatulence; and the tongue was most remarkable—it was precisely as if thickly coated with white paint, with a yellowish streak in the centre. She was very thin, and had a slight cough. On stethoscopic examination, a large dry cavity was found to exist in the upper part of the left lung. She improved considerably at first under the use of manganese, with small doses of morphia; but the improvement lasted only a few days. Arsenic was now prescribed, with the effect of entirely removing the pain; and she continued free from it for some weeks afterwards, during which she remained under observation.

These cases represent three classes, apparently of very different nature, in which arsenic was curative. But a close connection may be traced between them. A peculiar state of a nerve, call it exaltation or depression according to the theory adopted, is the cause of pain. And when pain occurs spontaneously, without inflammation, and is sudden in its coming and its going, we term it neuralgia. These attributes of neuralgia were well marked in the two first cases. In this class, experience leads me to say that it will hardly ever fail.

In the case dependent upon marsh-poison, as traceable from the previous ague and the state of the spleen, the inscrutable connection between ague and neuralgia suggested itself. This connection led to the exhibition of a remedy of known power against both.

In the case of phthisis, it is worthy of note that the pain was induced by food. But the failure of ordinary remedies for pain after food, as well as analogy, led me to try arsenic. Neuralgia is often developed in parts at a distance from the irritation upon which it depends. Thus, a carious tooth will produce it in the upper part of the face; or a foreign body, as a bullet or a splinter, in one part of the tissues will cause it in another part. The late Dr. Brinton, speaking of the kind of pain at present under consideration, says: "These cases of gastric phthisis may thus be regarded as a kind of neuralgia of the pneumogastric and sympathetic nerves; certain thoracic segments of these conjoined nerves forming the starting point of an irritation and morbid action which is transferred to their abdominal connections." (*Diseases of the Stomach*, second edition, p. 350.)

The contact of food with the stomach in the foregoing case, was the exciting cause of pain. But the effect of treatment alone distinguishes it from ordinary gastrodynia, in which arsenic is always injurious. One of the main indications that the pain in the first class of cases was neuralgic was its great severity. But the probability that gastric pain of the same nature, but of lesser degree of severity, existed, soon occurred to me. Acting upon this, I have treated with success a considerable number of cases of the more moderate kind by the same means. In proportion, however, as severity diminishes, difficulty of diagnosis increases, and here diagnosis is of the greatest importance. Care must be taken, lest the arsenic do harm instead of good. In the ordinary case of pain from intolerance of the mucous membrane of the stomach to the contact of food, arsenic acts as a direct irritant. The gastric pain, sense of heat, epigastric tenderness, and redness of the tip of the tongue, are speedily increased. In doubtful cases, the best rule is not to give arsenic until after due attention has been given to diet, and certain remedies have failed. Bismuth is one of the best known of these. But, if I may be allowed to speak here of another remedy of my own introduction into practice, I would add that the purified black oxide of manganese is superior to bismuth, except in cases to be presently mentioned. If the merits of both drugs were in other respects the same, manganese possesses one great advantage in not constipating the bowels, as bismuth almost invariably does. But, in a few cases, the irritability of the gastric mucous membrane is continued in that of the small intestine. The pain is then associated with a tendency to diarrhœa, and the astringency of bismuth becomes valuable. It is in such cases alone that I now employ it.

It will be well to set down here the cases of gastric pain, in order of frequency of occurrence, for which arsenic is unsuitable.

1. Unnatural sensibility of the stomach to the contact of food, except that which occurs in certain cases of phthisis.
2. Pyrosis, not of malarious origin.
3. Subacute gastritis.
4. Ulcer of the stomach.
5. Unnatural sensibility from gastric congestion, due to disease of the heart.
6. Cancer.

In the selection of cases for treatment with arsenic, certain things must be kept well in view. It should be clearly ascertained whether the pain is induced by taking food or not. If the pain be violent, and if it come on generally when the stomach is empty, and be not dependent upon one of the causes specified as being beyond its reach, the remedy will almost to a certainty succeed. If the patient live in a district where ague prevails,

and especially if he have had the disease, or if he have had neuralgia of the face or head, there are strong indications for the treatment. But if the papillæ of the tip of the tongue be red and prominent, if the epigastrium be constantly tender on pressure, if the skin be hot and dry and the pulse quickened, the remedy will not succeed.

The usual precaution of giving arsenic soon after a meal should be practised, and it is always proper to begin tentatively. If the drug do not disagree, the dose should be gradually increased. When large doses are reached, a few drops of tincture of opium may be desirable to prevent purging. The constitutional effect of arsenic must, of course, be attended to, as warnings to diminish or to omit its use.

Treatment of Gastrodynia.—[The following is from a letter to the Editor of the British Medical Journal by Dr. J. T. Campion, of Dublin. Dr. Campion says Dr. Leared's paper recalls to his mind the treatment of gastrodynia by Dr. Ross, Medical Officer of the Warrenpoint and Rostrevor Dispensaries.]

But his mode of administration was the arsenious acid, or common white arsenic in powder—one grain divided into twelve pills—one of which was taken after breakfast and after dinner, daily. Dr. Ross told me that he was in the habit of using this remedy very largely, and that he always found it act almost as a specific in most cases of distressing gastrodynia.

[Dr. Norris, of Stourbridge, says :]

For twenty years I have frequently suffered from tic in the face, and a chronic pain in the central bowels, continuing some hours without disorder of any function, except occasional flatulency, and yet it does not appear to have affected my health in any way, for at my age I am unusually healthy and active, with a flushed countenance, free from any tinge on the skin betokening organic disease. Some writers think neuralgia does not affect the health, and this appears in part proved by my case.

I read some excellent remarks on gastrodynia in the British Medical Journal a few weeks ago, which induced me to believe that I had been long suffering from neuralgia in the bowels ; in fact, I sometimes thought so, and had now and then found relief by taking arsenic and applying chloroform, but I never gave either of them a fair trial, and have of late trusted to a small dose of laudanum, or one cigar, and the pain has usually faded away. The pain has generally come on two or three hours after dinner ; and I at first attributed it to drinking water from a leaden pump many years ago ; and, as I and some of my

family have been subject to tic in the face, it is quite fair to infer that I have been long suffering from neuralgia in the bowels, and what makes it still more probable, for the last few weeks I have had very little pain in the face and much more pain in the bowels, till I began to take arsenic, as recommended by Dr. Leared, and now for some weeks I have scarcely had any suffering in the bowels, and I think I shall soon be free from complaint. I ought to remark that my abdominal pain generally came on during digestion; and it is probable that other persons subject to neuralgia would have it in an organ or structure predisposed to it when in a greater state of excitement. I took about five drachms of liquor arsenicalis, in doses of five or six drops twice a day. In less than a week, the pain began to diminish, and then gradually left me.

I feel much obliged to Dr. Leared for his valuable and interesting remarks; and I trust and hope the remedy will be fairly tried, and that hundreds of others will obtain the same relief that I have done.

It will probably give the remedy a fairer chance, if the digestive organs are at the same time carefully regulated; and should slight diarrhœa come on after taking it for a few weeks, a few drops of laudanum may be conjoined with each dose, and a light nutritious diet should be taken.—*Brit. Med. Journal*, *Nov.* 23, 30, *Dec.* 14, 1867, *and March* 28, 1868, *pp.* 467, 494, 560, 297.

26.—A CASE OF ENCEPHALOID DISEASE OF THE LIVER.

Under the care of Dr. WADHAM, at St. George's Hospital.

A case involving some points of interest in regard to cancerous disease of the liver was lately in this hospital. Of 83 such cases analysed by Frerichs, only 7 were in persons under thirty years of age. The age of this patient was twenty-eight. The rapidity of progress was unusually remarkable. As more often happens than not in cancer of the liver, there was no jaundice. Dr. Reginald Thompson, medical registrar, has obliged us with notes.

The patient, Henry H., was a carpenter, who was admitted on Nov. 14th, 1867. He had been in perfect health until five weeks before admission. His illness began with rigors, and pain in the right side of the thorax and in the liver. He had not been jaundiced.

On admission, he was suffering principally from dyspnœa. On being closely questioned he said he had a feeling of constriction

—of tight lacing—across the liver. This organ was found to be enormously enlarged; it extended from the level of the right nipple to below the umbilicus, and measured in this direction eleven inches; it was nodular and hard. There was no tenderness and no jaundice. The right lung was dull, and the sputa contained a large quantity of elastic tissue. The cervical veins were much distended. The disease was diagnosed to be encephaloid deposit in the liver and right lung. The patient died on the 22nd November, the liver having increased in size.

The post-mortem examination was made by Mr. Pick. Several masses of encephaloid cancer were found in the right lung and in the liver. This organ was of an enormous size, and weighed 210 ounces.—*Lancet, Jan.* 4, 1868, *p.* 12.

DISEASES OF THE URINARY ORGANS.

27.—PEROXIDE OF HYDROGEN AS A REMEDY IN DIABETES.

By Dr. John Day, Geelong, Australia.

[The patient was 36 years of age, and was progressively getting worse, passing as much as five quarts of highly saccharine urine during each night.]

While pondering over the hopeless condition of my patient, it occurred to me that if I could oxidise the sugar that had been taken up in the general circulation, it would be an approach towards the natural mode of its elimination by the lungs. With this object in view, I gave half-drachm doses of ethereal solution of peroxide of hydrogen mixed in an ounce of distilled water, three times a day.

To enable me fully to explain the theory on which I base my treatment, would occupy far more of your valuable space than I could justly claim. Schönbein believes that peroxide of hydrogen is H O + antozone, and that the blood-corpuscles possess, in a very high degree, the property of decomposing it, and of transforming its antozone into ozone, without, in themselves, undergoing any very rapid change; and he further believes that ozone is the only condition in which oxygen possesses any active combining properties. Assuming these views to be correct, we should possess in ethereal solution of peroxide of hydrogen, which would be rapidly absorbed, a ready means of destroying, by oxidation, the sugar in the blood, and of also maintaining the animal heat, which, in the treatment of diabetes, is an important consideration. I may observe, that what is sold by Mr.

Robbins as Dr. Richardson's ozonic ether is, in reality, a solution of peroxide of hydrogen in ether. This may be readily proved by adding a few drops of it to a weak solution of chromic acid: a beautiful blue colour will be the result, caused by the formation of perchromic acid. This preparation is in every respect similar to that which I have been using, and in the therapeutical effects of which I have now had some years' experience.

I commenced the use of this new remedy on August the 10th, and, as the following extracts from my case-book will show, with most gratifying results to the patient:

Aug. 12th. From 10 p.m. to 10 a.m., passed about five pints urine. Previously, for many months, the quantity of urine passed during the night averaged five quarts.

13th. Quantity of urine passed during the night, rather less than three pints and a half. Thirst not so urgent.

14th. Quantity of urine passed during the night, two pints and a half. Urine strongly acid; specific gravity 1046. Thirst much less urgent.

16th. Quantity of urine passed during the night, rather less than forty ounces. The patient very much improved in every respect. I give her own words:—"I have no thirst now; no more than I had in olden times. I feel that I am cured if it will only last."—*Lancet, Jan.* 11, 1868, *p.* 45.

28.—ON THE URINARY PIGMENTS.

By Dr. Henry Veale, Royal Artillery.

The plans which are ordinarily recommended for examining the urinary pigments are either very troublesome, or of such little practical value as to be but seldom had recourse to by the clinical physician. The method recommended by Dr. Payne (*Indian Annals*, Sept. 1858) was to boil a certain quantity of urine in a test-tube, and then to treat it with nitric acid, drop by drop. By this proceeding, the urine usually becomes darker with each addition of the acid until a maximum of colour is produced; after which, if more acid be used, the changes are retrogressive. Dr. Payne was of opinion that, in cases of hepatic disease, the colour thus produced was always much darker than in health. In my hands, however, this has not constantly been the case. In inflammatory diseases of the liver, I have frequently observed exactly the reverse; and notwithstanding many trials, I have been unable to determine that Dr. Payne's method is of any diagnostic value.

The simple proceeding of Heller and Carter of allowing a certain portion of urine to mix slowly in a test-tube with a smaller quantity of sulphuric or hydrochloric acid is of more value. I have not, however, been able to obtain sufficient uniformity in the results from the use of either of these acids. As tests available at the bedside, they are decidedly inferior to the strong nitric acid when properly employed.

Dr. Andrew Clark of the London Hospital has shown that there is no better mode of testing for small quantities of albumen than that of allowing a small quantity of urine to trickle slowly down the side of a test-tube upon about a half a drachm of fuming nitric acid. I have employed this test extensively, and fully agree with all that Dr. Clark has said as to its certainty and simplicity. I had not used it long, however, before I was struck with the difference in the appearance of the pigmentary band. By this term I mean the coloured rings which form at the line where the urine floats upon the acid. The quantity of nitric acid employed should always be sufficient to reach the portion of the test-tube, which is uniform in calibre. If the acid be poured into the tube on one side, the urine should be allowed to trickle down only on the opposite; and it will be found convenient to use a pipette for letting the urine flow into the tube. If more than a drop or two runs down at once upon the acid, there is apt to be a mixture of the fluids, which spoils the experiment. Inclining the tube at an angle of from 50 to 60 degrees, or even more, much facilitates the operation, but in restoring the tube to the vertical position care must be taken not to agitate the contents. To see the result, the tube should be held in a certain light, and generally it will be best to have a white background, such as a whitewashed wall or a sheet of white paper.

If the experiment has been properly performed, there will be found, when healthy urine has been employed, a yellow ring at the line of contact of the two fluids, and over this a narrow ring of an almost ruby-red colour. In many scores of observations, I have not yet met with an exception to this rule. In certain morbid states of the system, however, instead of the ruby ring, the urine strikes a purple, bluish, or absolutely blue ring. When bile is present, there is also a green ring. These coloured rings are produced immediately, and are at first very distinct, but they gradually cease to be visible as the mixture of the urine and acid progresses.

In the intermittent and remittent fevers of India the urine will frequently display the purple and blue rings. In nearly every case of dysentery that has come under my observation since my attention was drawn to this subject, I have found a

decidedly blue ring to be produced so long as the disease was progressive, and the ruby-red ring to reappear when the patient began to recover. I have not often seen a blue ring in cases of congestion and enlargement of the liver, unless there happened to be some other concomitant state, such as fever or diarrhœa, to which it might be ascribed. In a case of enlarged liver, anæmia, and consequent functional cardiac disorder, attended with much debility, which came under my notice some months ago, there was no movement of the bowels for six days, notwithstanding several doses of colocynth pill, calomel, and julep, castor-oil, &c. On the sixth day of the constipation, I prescribed one minim of croton oil, which occasioned one motion after the lapse of several hours. I then ordered the dose to be repeated, and the result was, that within the ensuing twenty-four hours the bowels were moved eight times. After the eighth motion, I examined the urine in the manner above described, and found a distinctly blue ring to be struck over the yellow. During the next twenty-four hours, there was no movement of the bowels, but the urine still presented the same appearance. On the third day the bowels were still constipated: the urine, however, no longer displayed the blue ring. On the fourth day, after a dose of pil. aloes and myrrhæ, the bowels were moved eleven times; the pigment then became of a dark purple colour, but not distinctly blue. From this time he improved, and the blue and purple rings ceased to be produced. From this case it would appear as if even artificial purgation were sufficient to induce the change in the condition of the urinary pigment.

This blue or purple pigment is of course no other than the uroglaucin of Heller, or the indigo-blue of Carter and Schunk; but although it may be possible, as the latter authors have shown, to obtain this substance from nearly every specimen of normal urine, it would be erroneous to conclude that the almost instantaneous formation of the blue ring, when urine and nitric acid are brought into contact in the manner above mentioned, is a normal or physiological change. Up to the present time, I have never seen it produced in the urine of healthy persons. As a temporary symptom in the course of a malarial fever, I have not observed it to indicate danger to life or the reverse; but in all cases hitherto in which it has been detected day after day, where it has been persistent, in short, the disease has taken a downward course.

Judging from the nature of the cases in which this condition of the urine is found (dysentery, severe malarial fevers, excessive purgation, &c.), I think it may be concluded that it is in direct relation with the amount of destruction or loss of the colouring matter of the blood, and with the inability of the system to

repair the waste at the time. Under normal conditions, it would appear that the supply of colouring matter to the blood somewhat exceeds the waste, but that, in certain diseases in which the loss of colouring matter is excessive, or in which the supply of the requisite materials cannot be duly profited by, there is an interruption in the process of pigment formation, which probably essentially consists in an insufficient amount of oxidation. Whatever may be the true explanation, it is to be hoped that those who are practising in cold or temperate climates, and who have to deal with diseases of a different kind from those usually witnessed in India, will give to the profession the result of their observations on this subject.—*Edinburgh Medical Journal*, *Dec.* 1867, *p.* 548.

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SURGERY.

BONES, JOINTS, ETC.—AMPUTATIONS, FRACTURES, DISLOCATIONS.

29.—ON FRACTURE OF THE THIGH.

By Lawson Tait, Esq., Resident Surgeon, Clayton Hospital, Wakefield.

[In the following paper the writer brings under notice the particulars of some cases in which he used a new plan of treatment of fracture of the thigh, with good results.]

The method referred to is that usually known as Buck's, and is described in Hamilton's elaborate treatise. Two years ago I had the good fortune to make the acquaintance of Dr. George K. Smith, of Brooklyn, U.S., who had been attached to one of the rear hospitals of the Federal army during the late war. Dr. Smith is the author of a most exhaustive paper on fractures of the neck of the femur, and on this account was allowed to select, as an especial study, cases of gunshot fracture of the thigh. His treatment consisted entirely in the use of Buck's method, and the success obtained by it has been extremely satisfactory. In the elaborate reports and catalogues issued by the Army Medical Department his cases are noticed; but I had the opportunity of examining photographs of many of his patients, and must express my opinion that the success obtained during that unfortunate war in the treatment of gunshot fractures of the thigh was such as immeasurably exceeded that of any other known campaign.

The principle on which the plan is based—that of continuous extension by a weight and pulley—is not new, but the credit of adapting it successfully to the treatment of fractures of the thigh is certainly due to the American surgeons. The following will be found a convenient method of applying Buck's treatment:—Having placed the patient on a convenient bed, fasten two strips of ordinary diachylon plaster (that spread on unglazed calico is best) reaching from about three inches above the knee to the malleolus, and from two to three inches wide, one on either side of the injured limb, and leave as much as will tie round a

piece of wood adapted to the sole of the foot. This foot-piece must have a hole bored in it about three inches from the heel, through which a cord is run from above downwards and secured by a knot; it must also have a flat notch on either side in which the plaster straps are laid. The cord ought to be in front of the straps. The limb is now to be carefully bandaged from the toes to the upper ends of the plaster straps, and the bandage to be saturated with dextrin or paraffin. The foot of the bed is to be raised about four inches to obtain counter-extension by the weight of the body. The cord is to be reeved over a pulley, and have a weight attached to it. The fragments having been adjusted, four Gouch splints are to be applied of suitable length on the four surfaces of the limb, and those are best retained in position by small straps buckled. A slight addition, which I have found of great importance in preserving the heel from injury, consists in having a small cradle placed over the foot, to which the foot is slung by a small cord running through a hole in the toe of the foot-piece ; this also prevents the tendency to eversion which the natural form and axis of the limb create. The more robust the patient the greater the extending weight required, and more weight is necessary during the first week than afterwards. In children I have had good results with so slight a weight as four pounds ; and in the case of a full-grown man I have had to use sixteen pounds. I have treated fractures of the thigh in all the various sites of fractures, and the result has been uniformly satisfactory. Of course, in cases of fracture close to or above the trochanter the splints on the thigh are unnecessary—the extension alone must be trusted to. It is not necessary that the splints should command the knee-joint. Indeed, except in cases when the fracture is very close to the condyles, I think it better that they should not do so. There is no object to be gained in restricting all motion of the hip-joint ; indeed, all the cases have been allowed, if they wished it, to sit up in bed after the first week.

The true indication for treatment in all fractures is to reduce the patient, as far as his injured part is concerned, to the condition of a crustacean, to supply him with a temporary dermoid skeleton until his vertebrate is repaired. The best illustration of the successful fulfilment of this indication is to be seen in the treatment of Colles' fracture by means of Professor Gordon's splint; and when we have equally efficacious means of treating every injury to the osseous system this department of surgery will be perfect.

The cases in which I have used this treatment are eleven in number—two compound and nine simple.

Besides the actually better results obtained from this method of treatment the patients are much more comfortable during its

continuance than under any other that I have seen. In cases where bedsores threaten they can be more easily obviated in this method than in any other.—*Dublin Quarterly Journal, Feb.* 1868, *p.* 71.

30.—ON THE FRACTURES OF THE PATELLA, TREATED BY A RING.

By Dr. Wm. A. Gibson, St. Louis, U.S.

[The account of the following case was first published in the St. Louis Medical and Surgical Journal, but it is taken by us from a pamphlet by Dr. Paul Eve.]

The patient, Mr. James Ferree, aged 35 years, had been the victim of rheumatism, and having so far recovered that he could use crutches, in attempting to get into a buggy, felt a sudden shock, as if some one had hit him with a stone on the knee, and could scarcely be convinced that the fracture was caused by muscular contraction.

The patella of the left knee was fractured transversely through its middle, the fragments separating about one inch. I did not admire any of the modes of treatment for the fracture which I had ever seen recommended or used, and while hesitating what plan I would adopt, it occurred to me that a ring would answer the purpose. I therefore took a measurement of the sound patella, and had a ring made of iron (allowing for padding), which I padded well with cotton wadding, cut in strips and wrapped around the ring, over which I applied a bandage. To each side of the ring I sewed strips of bandage. I then placed a well-padded splint twenty-four inches long to the posterior aspect of the leg and thigh, which I secured by a few turns of bandage at the lower and upper ends, the bandage being loose so as not to interfere with the circulation. I next brought the two fragments of bone into apposition, and placed the ring around the patella, and tied the strips of bandage over the splint thus securely holding the ring in its place, and keeping the broken bone always in complete apposition, thereby giving the greatest possible chance for a bony union. At the expiration of thirty days, I removed the ring, and commenced passive motion of the limb, and to-day (June 15th) he has very good use of the limb. During the time this case was under treatment, I invited Drs. E. H. Gregory, J. J. McDowell, E. A. Clark, E. S. Frazier, and other physicians, to examine the appliance, and they all expressed the opinion that it was *just the thing* for a fractured patella. The advantages of the ring in the treatment of this troublesome fracture are so apparent, that it seems to me hardly necessary to point them out. The appliance did not give the

patient the least pain, and there was no interruption of the circulation by the bandages. It was impossible in this case for the patella to escape from the ring, but possibly in some cases, as of women, when there is a good deal of adipose tissue, and but little prominence of the patella, it may not be so easy to apply the ring; but I am persuaded that it will give entire satisfaction in all cases. I claim by the application of the ring to have reduced one of the ugliest fractures of the human frame to one of the simplest for treatment.—*Pamphlet on Fracture of the Patella, p.* 4.

31.—A NEW APPARATUS FOR THE TREATMENT OF FRACTURES OF THE LIMBS.

By Christopher S. Jeaffreson, Esq., Senior Resident Surgeon to the Royal Free Hospital.

[In this paper Mr. Jeaffreson recommends the use of elongated water cushions, placed between the splints and the limb, the splints being applied just as usual. Three are required for the leg, one for either side and one for the under surface of the limb. The idea is a good one, on account of the evenness of the pressure produced, and the consequent comfort to the patient. We do not think it necessary to give the paper. The apparatus may be seen in use at the Royal Free Hospital.]—*Lancet, Nov.* 9, 1867, *p.* 576.

32.—ON THE TREATMENT BY MANIPULATION OF DISLOCATION OF THE THIGH BACKWARDS.

By George W. Callender, Esq., Assistant-Surgeon to, and Lecturer on Anatomy at St. Bartholomew's Hospital.

[The patient, whose case is related, was a robust labourer. The head of the femur lay low down in the ischiatic notch. Various attempts at reduction were made, but without success.]

Very much vexed at the failure of these attempts, I was at the time disposed to think that the position and extent of the rent in the capsule might be the source of difficulty. The capsule might be interposed, curtain-like, between the head of the femur and the acetabulum, or the rent in the capsule might be so situated that the head of the bone could not be brought opposite to, or directed so as to pass through it. But on looking more closely into the question, I am of opinion, from the evidence we possess, that the capsule could not in this instance, and indeed that it never can, offer any obstacle to the reduction of dislocations at the hip. As Sir Astley Cooper says, the idea

of the neck of the femur being girt or confined by the capsule is quite untrue; the violence, indeed, which causes the dislocation (I write only of dislocation the result of violence), and which forces the bone through the capsular ligament, ensures also that the latter shall be torn to pieces. An account of a rare form of dislocation on the tuberosity of the ischium has been given by Mr. Wormald who has also deseribed the specimen which is in the museum of our hospital. It may be referred to as showing the great damage sustained by the capsular ligament. In a dislocation of the femur upon the ischiatic notch, which occurred three weeks before death, the opening in the posterior part of the capsule is widely torn, although its size has been reduced by partial union. In a recent dislocation upon the lower edge of the obturator externus muscle the laceration of the capsule on the inner (obturator) and lower sides is extensive; and in a fourth case, one of dislocation in the ischiatic notch, the capsule is described as widely lacerated by the head of the bone, and the rent is so considerable that it is evident the ligament could offer no obstacle to the reduction of the displaced femur. I have looked through the specimens in the museums of several of the London hospitals, and I find that they show just the same state of the parts, tearing of the capsule to a great extent, and a free passage for the head of the bone into the acetabulum.

Dr. Fenner has, however, recorded an instance of a rent in the capsule (which was half torn through) through which the head of the bone could not be forced, even when the parts were tested at a post-mortem examination. Such a case must be exceptional. I do not consider our present pathological experience would justify us in thus explaining any failure in the reduction of a dislocated hip.

Not long ago I examined a recent dislocation at the shoulder, and found the head of the humerus driven through the subscapularis, so that the neck of the bone was tightly surrounded by muscular fibres; and I have seen with Mr. Wormald a dislocation of the radius forward which could not be reduced because, as we thought at the time, the bone had been forced through, and its neck nipped by the supinator radii longus. So also it is just possible that with luxation at the hip, muscular fibres may interpose and hinder the return of the head of the bone into its socket; thus in Wallace's case the edge of the glutæus medius passed over the head of the femur, and in a case related by Mr. Syme the head of the thigh-bone was imbedded in the substance of the glutæus maximus. I do not lay much stress upon the difficulties which may originate in such conditions as the preceding, but they are worth a passing notice, as also is the hindrance to reduction which has been known to result from locking of the head of the bone when deeply placed

in the ischiatic notch, as in one of the cases recorded by Mr. Wormald.

Sir Astley Cooper has described the difficulty which attends the reduction of dislocation on to the ischiatic notch, but I am not aware that any explanation of this difficulty has been offered, save the one which he refers to in describing the distance which the head of the bone has to travel outwards from the fossa of the ischium before it can be lifted over the edge of the acetabulum. Hence the stress which has been laid, since his observations were published, upon the importance of endeavouring to lift the head of the bone upwards and outwards whilst extension is being made.

There is, however, one very evident obstacle to the reduction of this class of dislocations to which attention may be directed, as it seems to give the only fair explanation of the difficulties which are experienced in the treatment by extension as well as by manipulation. When the head of the bone lying on the ischiatic notch is pulled forward by extension, it is drawn over the convex outer surface of the acetabulum; but instead of passing towards the socket it happens, from the formation of this part of the pelvis, that it can follow an easier route which is open to it. From the lower part of the great sciatic notch a broad, smooth, pulley-like surface leads to, and then curves round, the inferior border of the acetebulum, and thence ascends towards the obturator foramen. The obturator internus above, and the great sciatic nerve, and the upper border of the obturator externus below, rest within it; but between these the groove is covered with fat; and from its surface fibres of the capsular ligament take their origin. If the head of a femur be placed in its corresponding pelvis in this groove it will be seen how accurately the convexity of the head of the bone fits the concavity of the ischium. If also the head of the femur be placed on the sciatic notch, and is then moved towards the socket, it will be found that it slips over the convex outer surface of the acetabulum, and is directed into and along the groove just referred to; and this I believe must happen when extension is employed for the reduction of these dislocations—the head glides below the socket, and slips up again when extension is remitted. In attempting to replace the femur by manipulation, as this operation is usually practised, by the rolling outwards of the shaft, the head is also carried along this groove, its progress through which may be traced easily enough, and incurs the risk of passing round the acetabulum, so that the head, when the thigh is extended, comes up on the obturator side, or rises even to the pubes, having described a half circle below the socket. This rolling of the head round the acetabulum is a chief objection to the plan of reduction by

manipulation, and is almost sure to occur if the thigh is rotated outwards; in this way the ischiatic nerve has been seriously crushed, and the bruising of the parts has been such as to give rise, in one instance at least, to subsequent disease of the hip-joint.

To return to the case before us. After the patient had rested for several days I adopted the following plan :—The thigh was bent upon the abdomen, and I slowly moved the limb into a straight line with the body, so that the head of the bone could be felt projecting in the buttock, outside the tuber ischii. The limb, in a straight line with the trunk, without allowing any rotation outwards, was then drawn forward from the abdomen, and forced downwards (extended), and the head of the bone at once slipped into the acetabulum. These movements were made slowly and steadily, and the limb was extended with care, remembering the great leverage which we were making use of, and the position of the head, which was being pressed up into the socket. In two recorded cases the neck of the femur has been broken under a somewhat similar strain.

If these manœuvres are examined by the help of the skeleton, it will be found that by flexion, and by moving the thigh into a straight line with the body, the head is brought from the notch into the groove just above the outer side of the tuber ischii. Here it is opposite the least prominent part of the lower edge of the acetabulum, and if the femur is depressed whilst in this position the head easily slips into the socket. Dr. Markoe, with the addition of a rocking movement as the thigh is extended, and Dr. Hamilton, employ somewhat similar manœuvres, and they speak favourably of their success.

I have been anxious to give some explanation of the manner in which the method by manipulation acts, and to insist upon the importance of not abducting or rolling the limb outwards, for if this is done the head of the bone is almost certain to roll past the acetabulum to its inner side; or if an obturator dislocation is under treatment, and the thigh is rotated inwards, the head of the femur will, as I have several times seen it, roll round on to the ischiatic notch, just reversing the movement which takes place when an ischiatic dislocation is improperly manipulated.

Three steps complete the operation. First, the thigh is bent on the abdomen; secondly, it is brought into a straight line with the long axis of the body; thirdly, it is forced down (or extended) in a straight line, parallel with its fellow. The dislocation is thus reduced without difficulty and without the need of any assistant. And, what is of chief importance, the

operation avoids all risk of rolling the head of the bone round the acetabulum, an accident which is so apt to complicate manipulation as commonly practised.—*Lancet, March* 14, 1868, *p.* 343.

33.—ON A NEW METHOD OF REDUCING DISLOCATION OF THE HEAD OF THE HUMERUS.

At the Frankfort meeting of German Naturalists and Physicians, Dr. Heine, of Heidelberg described the following novel plan of reducing a luxation at the shoulder-joint. The case in which it was first applied with success, was one of subclavicular luxation of the humerus of seven weeks' standing. Extension and other means of reduction had been attempted, with the aid of chloroform; but with no success, as the head of the bone could not be lifted over the anterior border of the glenoid cavity. The elbow of the affected side being bent at a right angle, Dr. Heine grasped the arm with his right hand and the forearm with his left: he then raised the extremity to the vertical position and forced the elbow backwards, so that the upper arm formed an obtuse angle with the posterior surface of the neck (hyper-elevation and retroflexion). The humerus was thus converted into a lever of the second form. The scapula formed the fulcrum upon which the surgical neck of the humerus rested, and the head forming the short arm of the lever was, whilst the shoulder was forced by an assistant, readily brought up to the level of the articular surface of the scapula. The arm was then circumducted from behind forwards—the head of the patient being turned to the opposite side; and as it was finally depressed and rotated inwards, the head of the humerus slipped audibly into the articular cavity. The action of the limb was now free, and the patient in a short time was able to perform nearly all the normal movements.—*Deutsche Klinik,* 46, 1867.—*Brit. Med. Journal, Feb.* 22, 1868, *p.* 168.

34.—CASES OF WOUNDS INTO JOINTS.

Under the care of JOHN BIRKETT, Esq., at Guy's Hospital.

Penetrating wounds of the large joints always excite alarm in the mind of the surgeon. Even incised wounds, most carefully made, and with every precaution adopted to prevent inflammation, as for the removal of loose cartilages from joints, have been attended with disastrous consequences. Wounds inflicted by rough tools, and complicated with injury to the surrounding parts, must always be attended with corresponding risk to the integrity of the injured member and the life of the

sufferer. Taken in time, however, and carefully treated with the appliances of modern surgery, the danger accompanying these serious injuries may at least be averted, and, as the following cases illustrate, the usefulness of the injured joint need not necessarily be compromised. The principles upon which the treatment of the injuries was carried out were as follows: the immediate closure of the wound, the adoption of perfect repose of the body and immobility of the injured member, the local application of ice, and a moderate but nourishing diet.

Case 1.—*Incised wound into the knee-joint.*—The report taken by Mr. E. N. Edwards, the dresser of the case, is as follows:—On Feb. 8th, 1867, a healthy carpenter, 25 years old, fell from a height of several feet whilst carrying a basket of tools, with the sharp edge of one of which a wound was inflicted over the outer side of the left knee-joint. It was partly over the patella, and extended obliquely backwards and downwards about an inch and a half as the limb reposed in the extended posture. The edges of the wound were carefully kept in contact by adhesive plaster and a compress of lint, but synovial fluid escaped freely from the wound the day after the accident. The surface of the whole joint was warmer than that of the opposite side, and the capsular ligament was distended with fluid. The limb was fixed on a straight back splint, ice applied over the joint, and a moderate but nourishing diet allowed.

The progress of this case was satisfactory; for neither local inflammation nor constitutional disturbance to excite apprehension arose, and the upper half of the wound healed in a few days. Synovial fluid continued to ooze from its posterior termination, and retarded the healing process. Ice was continued for twelve days, when the effusion into the joint had subsided. The wound was not covered, and it seemed to heal more quickly under this condition, an incrustation forming over it, beneath which cicatrisation advanced.

The man left the hospital forty-eight days after the infliction of the injury quite well.

Case 2.—*Lacerated wound of leg communicating with the knee-joint.*—The following case is reported by the dresser, Mr. J. A. Sharp:—A healthy, hard-working man, 21 years old, was admitted on August 23rd last into Accident ward, in consequence of an injury received about an hour before, and produced by the kick of a horse. The calkin of the horse-shoe had struck the man just below the left knee-joint, the articulation at the moment being flexed. A wound was inflicted in front of the leg, over which a surgeon quickly placed a pad of lint and plaster. As blood flowed from beneath this dressing, it was removed, and a contused lacerated wound of the integuments

was seen. It was somewhat quadrilateral, oblique in direction, and about three-quarters of an inch in diameter, and situate two inches and a half below the anterior edge of the upper articular surface of the tibia, a little below the tubercle of the tibia. The soft parts were cut through to the surface of the tibia. The knee-joint was swollen, and when pressure was made over the patella, the blood flowed more freely from the wound. The last observation led to a very careful examination of the injury by Mr. Birkett, who found that there was not only blood within the synovial capsule, but also air; for upon continuing the compression, bubbles of air came out of the wound with the blood, and upon percussing over the swollen joint, the peculiar dull sound of a circumscribed collection of air beneath the integuments was recognisable by all the bystanders. It was not possible to expel all the air; but the size of the distended capsule was much reduced by expressing the blood from within it. The diagnosis was then clearly established, that from the integumental wound a free passage extended through the synovial membrane into the knee-joint, that blood flowed into its cavity, and that air had also gained admission therein. The limb was placed in an extended posture, and kept so by means of sand-pillows. Pressure was made over the joint to expel as much of the blood and air as possible; a compress of lint was strapped over the track of the injury *between* the wound and the head of the tibia, and water-dressing was applied on the wound itself. In spite, however, of this local treatment, the joint soon became again swollen, probably from effused blood. The character of the bleeding was venous. Strict repose was enjoined, ice was kept constantly in contact with the whole anterior and lateral surfaces of the joint, and a moderate diet only was permitted.

There was not an untoward symptom in the progress of this man's recovery. The wound healed in about a fortnight; the joint continued swollen, but was never painful; and he was very unwilling to follow the strict rules imposed as regards the absolute repose of the member. Ice was employed for three weeks, it being continued after the external wound was healed. For five weeks the man was kept in bed, and appliances used to maintain extension of the joint; and when he left the hospital, forty-three days after the infliction of the injury, the movements of the joint caused no pain.

The patient called at the hospital on Dec. 2nd, and stated that he did not experience the slightest inconvenience from the injury.

Case 3.—*Wound of the elbow-joint, complicated with fracture of the olecranon.*—This patient was admitted on the 4th April last, and from the report of the case by Mr. C. H. W. Parkinson it

appears that the man, 20 years old, was struck at by a fellow-workman with an ordinary carpenter's hand-saw, the back edge of which was very thin and sharp. With this weapon a wound was inflicted transversely across the posterior region of the left elbow-joint. The integuments were divided for about two inches in length, rather obliquely; and three-quarters of an inch of the olecranon ulnæ was cut off, which retained its ligamentous and tendinous attachments, and allowed the cavity of the joint to be seen during its flexion. Three sutures were employed to adjust the wound of the skin, and the arm was placed on a straight splint, well padded at the elbow. Moist lint was laid over the wound. After a day or two the wound became inflamed, and the man complained of pain. Ice was applied over the joint, which reduced the inflammation, although the wound assumed a sloughing aspect. The sutures were removed, and a lotion with permanganate of potash was applied over the wound. An angular screw splint was substituted in place of the straight one, but this, even, caused pain and swelling of the arm. In a few days it was removed, and the arm steadied by pillows. Ice was used for thirty days. After forty-six days the wound was quite healed, but still the joint was kept perfectly at rest, somewhat flexed. He left the hospital in sixty-nine days after receiving the injury, the member saved, but the movements of the joint impaired.—*Lancet, Jan.* 4, 1868, *p.* 11.

35.—ON THE ANTISEPTIC SYSTEM OF TREATMENT IN SURGERY.

By Joseph Lister, Esq., F.R.S., Professor of Surgery in the University of Glasgow.

The cases in which this treatment is most signally beneficial are divisible into three great classes—incised wounds, of whatever form; contused or lacerated wounds, including compound fractures; and abscesses, acute or chronic—a list, indeed, which comprises the greater part of surgery. In each of these groups our aim is simply to prevent the occurrence of decomposition in the part, in order that its reparatory powers may be left undisturbed by the irritating and poisoning influence of putrid materials. In pursuing this object we are guided by the "germ-theory," which supplies us with a knowledge of the nature and habits of the subtle foe we have to contend with; and without a firm belief in the truth of that theory, perplexity and blunders must be of frequent occurrence. The facts upon which it is based appear sufficiently convincing. We know from the researches of Pasteur that the atmosphere does contain among its floating particles the spores of minute vegetations and in-

fusoria, and in greater numbers where animal and vegetable life abound, as in crowded cities or under the shade of trees, than where the opposite conditions prevail, as in unfrequented caves or on Alpine glaciers. Also it appears that the septic energy of the air is directly proportioned to the abundance of the minute organisms in it, and is destroyed entirely by means calculated to kill its living germs—as, for example, by exposure for a while to a temperature of 212° Fahr., or a little higher, after which it may be kept for an indefinite time in contact with putrescible substances, such as urine, milk, or blood, without producing any effect upon them. It has further been shown, and this is particularly striking, that the atmosphere is deprived of its power of producing decomposition as well as organic growth by merely passing in a very gentle stream through a narrow and tortuous tube of glass, which, while it arrests all its solid particles, cannot possibly have any effect upon its gases : while conversely, "air dust" collected by filtration rapidly gives rise simultaneously to the development of organisms and the putrefactive changes. Lastly, it seems to have been established that the character of the decomposition which occurs in a given fermentable substance is determined by the nature of the organism that develops in it. Thus the same saccharine solution may be made to undergo either the vinous or the butyric fermentation, according as the yeast plant or another organism, described by Pasteur, is introduced into it. Hence we cannot, I think, refuse to believe that the living beings invariably associated with the various fermentative and putrefactive changes are indeed their causes. And it is peculiarly in harmony with the extraordinary powers of self-diffusion and penetration exhibited by putrefaction that the chief agents in this process appear to be "vibrios" endowed with the faculty of locomotion, so that they are able to make their way speedily along a layer of fluid such as serum or pus.*

Admitting, then, the truth of the germ theory, and proceeding in accordance with it, we must, when dealing with any case, destroy in the first instance once for all any septic organisms which may exist within the part concerned ; and after this has been done, our efforts must be directed to the prevention of the entrance of others into it. And provided that these indications are really fulfilled, the less the antiseptic agent comes in contact with the living tissues the better, so that unnecessary disturbance from its irritating properties may be avoided.

* I have seen vibrios, so minute as to be only just discernible with the highest power of an excellent microscope, shoot across the field of view with a velocity that astonished me.

The simplest conditions are presented by an unopened abscess. Here as no septic particles are present in the contents, it is needless to apply the antiseptic directly to the part affected. All that is requisite is to guard securely against the possibility of the penetration of living germs from without, at the same time that free escape is afforded for the discharge from within. When this is done we witness an example of the unaided curative powers of Nature as beautiful as it is, I believe, entirely new. The pyogenic membrane, freed from the operation of the stimulus derived from the presence of the pus pent up within it, without the substitution of the powerful stimulus of decomposition as has heretofore been the case after the opening of abscesses, ceases at once to develope pus-corpuscles, and, exuding merely a little clear serum, rapidly contracts and coalesces, discharging meanwhile its unirritating contents completely, whether the outlet be dependent in position or otherwise. At the same time the irritative fever and hectic hitherto so much dreaded in large abscesses are, with perfect security, entirely avoided.

In suppurations of the vertebræ or of the joints the results of this system are such as I ventured with trembling hope to anticipate; patient perseverance being rewarded by a spontaneous cure in cases where excision, amputation, or death must have resulted from any other known system of treatment. In short, the element of incurability has been eliminated from Caries.

In compound fractures and other severe contused wounds the antiseptic agent must in the first instance be applied freely and energetically to the injured parts themselves, the conditions being the opposite of those in unopened abscesses. The wound being of complicated form, with its interstices loaded with extravasated blood, into which septic organisms may have already insinuated themselves during the time that has elapsed before the patient is seen by the surgeon, mere guarding of the external orifice, however effectually, is not sufficient. After squeezing out as much as possible of the effused blood, a material calculated to kill the septic particles must be introduced into the recesses of the wound; and if the substance employed is of sufficient strength to operate to a certain extent as a caustic, this is regarded as a matter of little moment in comparison with the terrible evil of inefficiency in its antiseptic action. For experience has abundantly shown that parts killed in this way, including even portions of bone, become disposed of by absorption and organisation, provided that the subsequent part of the treatment is properly managed.

Sloughs, as ordinarily observed, are soaked with the acrid products of decomposition, and therefore produce disturbance

upon the tissues around them, leading first to their gradual transformation into the rudimentary structure which when met with at the surface of a sore, is termed "granulations," and afterwards to the formation of pus by the granulations. But a dead portion of tissue, if not altered by adventitious circumstances, is in its proper substance perfectly bland and unirritating, and causes no more disorder in its neighbourhood than a bullet or a piece of glass, which may remain embedded in the living body for an indefinite period without inducing the formation of pus; while the dead tissue differs from the foreign bodies alluded to in the circumstance that the materials of which it is composed are susceptible of absorption.

Antiseptic substances, being, like the products of decomposition, chemically stimulating, will, like them, induce granulation and suppuration in tissues exposed for a sufficient length of time to their influence; but there is this all-important difference, that an antiseptic merely stimulates the surface to which it is applied, becoming diluted and weakened by the discharge which it excites; but the acrid salts which results from putrefaction are perpetually multiplied and intensified by self-propagating ferments, so that every drop of serum or pus effused through their agency becomes a drop of poison, extending its baleful influence both in the injured part and in the system generally.

These pathological considerations indicate the after-treatment in compound fracture, and explain the progress of the case. The antiseptic introduced into the wound is soon washed out by the discharge or carried away by the circulation, so that the blood and sloughs at first imbued with it become unstimulating and amenable to absorption, while at the same time they are prone to decomposition should any living atmospheric germs gain access to them. The further treatment, therefore, must consist in maintaining an efficient antiseptic guard over the orifice of the wound until sufficient time has elapsed to ensure complete consolidation of the injured parts.

The sanious and serous discharge which occurs at the outset will give place in a few days to a small amount of pus, if the wound is dressed in such a way that the antiseptic continues to act upon the raw surface. This discharge, due to the stimulating nature of the application, being merely superficial, and involving no inflammatory or febrile disturbance, will occasion no anxiety to one who understands its cause; and I venture to repeat the caution given in a previous communication, that the surgeon must on no account be induced to explore the wound and pry into the source of the suppuration, so long as all is going on well otherwise; for such a course, by admitting germs

into the interior, may produce the most disastrous consequences in an otherwise promising case.

But although suppuration resulting from the stimulating influence of the antiseptic is no cause for anxiety, it is more convenient that it should be avoided; and this may often be done entirely by leaving the lower layers of the dressing permanently on the limb and changing only its superficial parts—a plan which, while it protects the wound against the introduction of mischievous particles, permits the foreign body in contact with the tissues to part with its antiseptic material and become an unstimulating crust, under which complete healing by scabbing may occur in wounds of a size hitherto regarded as inconsistent with this process in the human subject.

Upon these principles a really trustworthy treatment for compound fractures and other severe contused wounds has been established for the first time, so far as I am aware, in the history of surgery. In an hospital which receives an unusually large number of patients suffering from machinery accidents, and in wards which, from circumstances to which I need not here allude, were peculiarly unhealthy, my experience of compound fractures in the lower limb was formerly far indeed from satistory, even in the selected cases in which alone I attempted to save the limb. But since the antiseptic principle has guided us not only have ordinary cases of this formidable injury been treated by my successive house-surgeons with unvarying success, but limbs such as I should once have condemned without hesitation have gone on to complete recovery without either local or constitutional disturbance: a statement which might be suspected of exaggeration were it not that it refers to proceedings in a public hospital, witnessed not only by students, but by gentlemen once my pupils, and now practitioners in Glasgow.—*Lancet, Nov.* 30, 1867, *p.* 668.

36.—ON THE ANTISEPTIC PRINCIPLE OF TREATMENT IN SURGERY.

By Prof. James Syme, Surgeon to the Queen in Scotland.

The antiseptic system, in order to be employed with advantage, must be carefully studied and fully understood, theoretically as well as practically. The preparations employed by Mr. Lister, which have been adopted here, may be denominated carbolic oil, carbolic lotion, and carbolic paste. The composition of the first is carbolic acid and boiled linseed or other fixed oil, in the proportion of one to five; that of the second, carbolic acid and water, in the proportion of one to thirty; and that of

the third, carbolic oil with whitening, in the proportions requisite for the consistence of soft putty.

Case 1.—*Parotid Tumour.*—Mrs. T., aged 32, from Aberdeen, recommended to my care by Dr. Kerr, one of the surgeons to the Royal Infirmary of that city, was admitted on the 20th of November last for the removal of a tumour occupying the whole of the parotid region. It was of ten years' duration, firm consistence, and nodulated form, hardly admitting of motion, but very distinctly defined. On the 26th I performed the operation without any difficulty or injury of the *portio dura*, twisted the vessels, sponged the wound with carbolic lotion, stitched the edges together by silver sutures, and applied a plaster of carbolic paste. On the third day the wound was found to be quite healed; on the fourth the stitches were taken out; and on the eighth the patient went home.

There are few wounds less apt to heal by the first intention than that which results from the excision of a parotid tumour. The depth and irregular form of the cavity, together with the oozing of blood from the glandular texture, which must be cut more or less during the operation, sufficiently account for the difficulty thus experienced; and the perfect union, without a drop of matter, which has just been related, may therefore be regarded as very satisfactory testimony in favour of the antiseptic treatment combined with torsion.

Case 2.—*Wound of the Knee-joint.*—On the morning of August 10th, J. D., a farm servant, aged 53, while going out to cut grass in the neighbourhood of Carnwath, about thirty miles from Edinburgh, fell upon his scythe, which inflicted a severe wound on the right knee. He was put into a cart and conveyed to the nearest railway station, whence he travelled to Edinburgh, and was taken in a cab to the hospital. It then appeared that a wound, about four inches in length, extended obliquely across the knee, dividing the quadriceps extensor tendon, and affording free access to the joint. Carbolic oil was freely applied, by sponging the cavity; the cut edges, which had been widely separated, were brought together by sutures; a pledget of carbolic oil was placed over the wound; and a splint was applied to keep the limb straight. Not the slightest constitutional or local disturbance followed, the pulse never being beyond sixty-three, and on the fourth day, the wound being quite healed, the stitches were removed. On the 13th of September the patient left the hospital with the limb strong and flexible.

There are few more serious injuries than wounds of the knee-joint, even under the most favourable circumstances; and, when aggravated by their large extent, rudeness of the weapon that caused them, or roughness of the treatment to which they are exposed, they still more endanger the patient's limb and life.

The very satisfactory result of a case in which all the adverse conditions were so conspicuously present, must therefore be considered a very remarkable, if not unprecedented, occurrence.

Case 3.—*Compound Fracture.*—J. P., a boy, aged 5, was admitted on the 30th of September, with his leg so seriously injured by having been caught between the spokes of a cart-wheel, that it seemed at first sight to admit only of amputation. There was a compound fracture of both bones, and, in addition to this, a wound of the integuments and muscles almost completely surrounding the limb at a higher part. The bones were much displaced, and the soft parts severely bruised. Chloroform having been administered, the carbolic lotion was freely injected between the broken ends, and lint soaked in it was applied over the wound. The bones were then carefully adjusted, and retained in their proper position by lateral splints. There was not the slightest constitutional disturbance, or any discharge of matter from the cavity, to the orifice of which carbolic paste had been regularly applied, and the limb is now perfectly straight and strong.

Case 4.—*Compound Fracture.*—A. L., a woman, aged 25, was admitted on November 18th, soon after sustaining a compound fracture of the tibia and fibula by falling down a stair. The carbolic lotion was injected between the broken ends of the bones; the carbolic paste was placed over the wound; and lateral splints were applied after the fracture had been properly set. There was no constitutional disturbance, and no discharge of matter from the cavity. The limb is now perfectly strong and straight.

The most remarkable feature of the last two cases, is the total absence of fever and purulent discharge from the cavity; the wound, in the course of a few days, ceasing to have any communication with the broken bones, and becoming quite superficial, so as to require merely the ordinary water-dressing.

Case 5.—*Psoas Abscess.*—W. F., a man, aged 47, was admitted on October 28th, with a very large psoas abscess completely distending the triangular hollow of the thigh, and having the femoral vessels running over it. The patient stated that, about ten months previously, he had sustained a severe wrench of the spine in the lumbar region, and subsequently suffered so much pain at this part as to be confined for seven weeks to bed. Soon afterwards, he observed a small swelling in his groin, which had gradually increased. On the 11th, under the protection of lint soaked with carbolic oil, I opened the abscess by a free incision, and discharged more than a pint of purulent matter. The carbolic paste, spread on tinfoil, was then applied over the wound. Not the slightest constitutional disturbance followed; and there

was no discharge of matter from the cavity, there being merely a slight serous oozing, which gradually disappeared.

Not long ago, I should have declined to open this abscess, from feeling very sure that the effect of doing so would be a profuse discharge of matter, rapid emaciation, hectic irritation, irritative fever, and death; but now, even when there is reason to believe that the matter proceeds from diseased bone, the confidence derived from experience has removed all such fears; and, caries being regarded as the effect instead of the cause of suppuration, patients are no longer left to their fate under circumstances that would formerly have been deemed entirely hopeless.

Case 6.—*Chronic Abscess of the Mamma.*—C. A., a married woman, aged 25, was admitted on December 2nd, for a deep-seated tumour of the mamma, which, she said, had existed for twelve months. Having ascertained that it was an abscess, I made a free incision, and discharged six or eight ounces of purulent matter. As it then appeared that a considerable arterial branch was bleeding, and could not conveniently be either tied or twisted, a needle was passed through the wall of the cavity so as to convey a thread round the vessel on each side of the wound, which, being tied, prevented any further hemorrhage. The wound having then been sponged with the carbolic lotion, was covered with the paste. On the 6th—that is, two days afterwards—the ligatures were removed, and neither then nor subsequently was there any discharge whatever, either serous or purulent. On the 9th, the patient was dismissed; and, at the end of a fortnight, she returned to show that the breast remained perfectly well. There are few surgeons who have not encountered great difficulty in treating abscesses of the breast, and who will not be able to appreciate the value of this extraordinary result.

Case 7.—*Acute Abscess.*—T. D., man, aged 59, was admitted on December 4th, with a very large abscess of the forearm extending from the elbow to the wrist, which, he said, had commenced about a fortnight before, and had been very painful. On the 5th, a free incision was made, under the protection of carbolic oil, and nearly a pint of purulent matter discharged; after which the paste was applied over the wound. On the 8th, there was a little serous oozing; on the 10th, it had nearly ceased; and on the 12th, the cavity was completely consolidated.

From such cases as those which have just been related, it is evident that there has taken place in surgical practice an improvement which promises to produce a great diminution of human suffering and danger. Some attempts have been made, anonymously and otherwise, to filch away from Mr. Lister the credit justly due to him for devising and establishing the anti-

septic system, by representing the use of carbolic acid previously for other purposes as an anticipation of his treatment. But, although the agent was not new, the principles of its employment, the modes of its application, and the results of its effects, being so entirely original, I venture to hope that the members of my profession will no longer tacitly sanction such disingenuous and ungenerous conduct.—*British Medical Journal, Jan.* 4, 1868, *p.* 1.

37.—ON THE TREATMENT OF WOUNDS UPON THE ANTISEPTIC AND SUBCUTANEOUS PRINCIPLES.

By William Adams, Esq., Surgeon to the Great Northern and Royal Orthopædic Hospitals, &c.

[There is no novelty in the application of antiseptics to open wounds, and even the antiseptic which Professor Lister has selected, viz., carbolic acid, had been extensively employed by some few surgeons. There is, however, great novelty and originality in the method of applying the antiseptic adopted by Mr. Lister, as based upon the "germ theory" of M. Pasteur.]

The theory of M. Pasteur is simply this—that instead of regarding the septic or decomposing properties of the atmosphere as due to the oxygen and moisture, as generally believed; the decomposing properties are supposed to depend upon the universal diffusion through the atmosphere of minute organic molecules, which, by their devolopment in the blood or serous exudation in wounds in which they are deposited, give rise to fermentative and putrefactive changes.

According to this theory, then, minute organisms constantly floating in the air are the immediate cause of putrefaction, and putrefaction is regarded as the cause of suppuration. Mr. Lister observes:—"We know from the researches of Pasteur that the atmosphere does contain among its floating particles the spores of minute vegetations and infusoria, and in greater numbers where animal and vegetable life abound, as in crowded cities or under the shade of trees than where the opposite conditions prevail, as in unfrequented caves or on Alpine glaciers . . . and it is peculiarly in harmony with the extraordinary powers of self-diffusion and penetration exhibited by putrefaction that the chief agents in this process appear to be 'vibrios' endowed with the faculty of locomotion, so that they are able to make their way speedily along a layer of fluid such as serum or pus." And in a footnote to this Mr. Lister observes:—"I have seen vibrios, so minute as to be only just discernible with the highest powers of an excellent microscope, shoot across the field of view with a velocity that astonished me."

The best general account of Mr. Lister's views is given in a paper read by him at the annual meeting of the British Medical Association in Dublin on August 9, 1867, and reported in the British Medical Journal, September 21, 1867. In this paper he observes—"But when it had been shown by the researches of Pasteur that the septic property of the atmosphere depended not on the oxygen, or any gaseous constituent, but on minute organisms suspended in it, which owed their energy to their vitality, it occurred to me that decomposition in the injured part might be avoided *without excluding the air* by applying as a dressing some material capable of destroying the life of the floating particles." . . .

"The material which I have employed is carbolic or phenic acid—a volatile organic compound which appears to exercise a peculiarly destructive influence upon low forms of life, and hence is the most powerful antiseptic with which we are at present acquainted. . . .

"In conducting the treatment, the first object must be the destruction of any septic germs which may have been introduced into the wound, either at the moment of the accident or during the time which has since elapsed. This is done by introducing the acid of full strength into all accessible recesses of the wound by means of a piece of rag held in dressing forceps, and dipped in the liquid. This I did not venture to do in the earlier cases; but experience has shown that the compound which carbolic acid forms with the blood, and also any portions of tissue killed by its caustic action, including even parts of the bone, are disposed of by absorption and organisation, provided they are afterwards kept from decomposing."

Such, then, is the general outline of the "germ theory" of M. Pasteur, and the principles of the antiseptic treatment based upon it by Mr. Lister. Further details of the treatment are given by him, and a considerable number of favourable cases, including severe compound fractures, lacerated wounds, abscesses connected with carious bone, &c., are recorded by him in support of the treatment.

The facts placed on record by Mr. Lister are of course beyond question, and are, no doubt, a faithful record of cases treated upon the antiseptic principle advocated by him, and these results show undoubtedly a larger amount of success in the treatment of severe compound fractures and lacerated wound than is generally met with. But still I do not feel inclined to accept Mr. Lister's explanation of these successful cases as based upon the "germ theory" and caustic antiseptics. I believe, on the other hand, that for these brilliant results Mr. Lister is mainly indebted to the adoption of the subcutaneous principle, the patients under the advantages of this system recovering even

from the effects of caustic antiseptics in addition to the injuries sustained. The phenomena observed by Mr. Lister, and explained by him as depending upon the antiseptic principle, to my mind admit of being equally well explained upon the subcutaneous theory; and there can be no doubt that equally good results, as to the healing of compound fractures and large lacerated wounds, have been obtained by other surgeons who have adopted the subcutaneous principle, combined with weak and unirritating antiseptics.

One clinical fact which to my mind appears to be opposed to the "germ theory" is the frequency with which wounds, under apparently unfavourable circumstances, heal by the first intention, without inflammation or suppuration, which we should expect would more generally occur if suppuration depended upon organic molecules always floating in the air, and therefore always ready to enter wounds where they would germinate and promote putrefaction. We must be careful to avoid assuming too much credit for any artificial means employed, and also to avoid the common error of attaching too little importance to the *vis medicatrix naturæ*, or the true reparative power dependent upon the healthy condition and constitutional powers of the patient.

The practical question is whether antiseptics are to be employed upon the "germ theory" of M. Pasteur, and applied of caustic strength to the deep recesses of recent lacerated and contused wounds of large size, such as in severe compound fractures, at the risk of causing further destruction of tissues and necrosis of bone; or whether they are to be used simply as antiseptics in a mild and unirritating form, with the sole purpose of preventing decomposition and putrefaction, such as would arise from exposure to the oxygen and moisture contained in the air, the healing of the wound being conducted essentially according to the subcutaneous principle—*i.e.*, the exclusion of the air by the mildest and least irritating means. My own opinion is strongly in favour of the latter view, and opposed to the application of caustic antiseptics.

This can only be determined when time has afforded other surgeons sufficient opportunities for deciding whether mild and unirritating antiseptics, applied to large wounds in combination with the subcutaneous principle of treatment, yield results equally as good as those reported by Mr. Lister as obtained by means of caustic antiseptic applications. I confidently anticipate they will do so, and observe that in a series of seven cases, including several severe accidents and injuries, treated by Mr. Syme upon the antiseptic principle, and recorded in the British Medical Journal of January 4 of the present year, no reference is made to the employment of caustic antiseptics. Mr. Syme

observes:—"The preparations employed by Mr. Lister, which have been adopted here, may be denominated carbolic oil, carbolic lotion, and carbolic paste. The composition of the first is carbolic acid and boiled linseed or other fixed oil, in the proportion of one to five; that of the second, carbolic acid and water, in the proportion of one to thirty; and that of the third, carbolic acid with whitening, in the proportions requisite for the consistence of soft putty." In these cases the wounds were sponged with the weak carbolic lotion, which, in the cases of compound fracture, was also freely injected between the broken ends of the bones, and lint soaked in it applied over the wound, which was also covered with the carbolic paste. In some cases the carbolic oil was used instead of the lotion. The results were, in all the cases, unexceptionally good, and I cannot but doubt that this practice will be generally followed, embodying, as it does, in a scientific manner the combination of the antiseptic with the subcutaneous principle.

To a certain extent it may be said that the practice is the same—viz., to exclude the air from the wound—whether this be done upon the "germ theory" or upon the theory of oxygen and moisture causing decomposition; but if the "germ theory" leads of necessity to the application of caustic antiseptics, then a wide difference in practice is at once established.

There can be no doubt that the greatest improvement in the treatment of wounds which has taken place in modern times, and that to which many lives are indebted for their existence, and by which many limbs have been saved, is the introduction of the antiseptic principle. Antiseptic lotions have now taken the place of water in the ordinary dressing, washing, and cleansing of wounds. We have, however, yet to determine by experience which shall prove to be the most efficient application, and amongst those generally in use are, the lotion of carbolic acid in the proportion of one part of the acid to thirty of water, or even weaker; Condy's fluid, which during the late wars proved so useful in army practice, and has long been in use in our general hospitals; and lime-water, diluted with equal parts of water, which, in inflamed and suppurating wounds, I have been in the habit of using for many years.

Whilst fully admitting the importance of the antiseptic principle, with the exception only of the application of antiseptics strong enough to act as caustics, such as the strong carbolic acid applications sometimes used, I am desirous of submitting to the members of this society that there are other applications and methods of dressing wounds of known efficiency; some of these owe their efficiency more to the subcutaneous than the antiseptic principle, whilst others depend more upon their

combination in equal proportions of the antiseptic with the subcutaneous principle.

As examples of the first class in which the subcutaneous principle is chiefly relied upon, I would mention, first, the method of dressing wounds simply with their own blood and dry lint. In this method the great object is to avoid exposure, or, at any rate, long exposure, of the cut surfaces to the air; and in this way, if no water be used, the wound being dressed only with dry lint, and a light pressure maintained by a bandage, incised wounds of considerable size, such as remain after the removal of moderate-sized tumours, amputation of fingers, &c., may be brought into a condition nearly as favourable to the reparative process as in true subcutaneous injuries and operations. All surgeons are familiar with the fact that large incised wounds, when treated in this way, as they sometimes are by patients in the absence of medical aid, frequently heal by the first intention, which would not have happened had the wounds been long exposed to the air, or if they had been washed with water.

Few practitioners have recognised the importance of excluding the air from open wounds, and still less have surgeons recognised the decomposing influence of water; but I may mention that for some time I have been in the habit of washing wounds with ether, instead of water, which I have found to contribute to the healing of many lacerated wounds, which appeared unlikely to heal by the first intention. Hippocrates, when speaking of the treatment of compound fracture, says:—"The wound is to be dressed in the summer with compresses soaked in wine; in the winter they should be dipped in oil, and the dressings will need renewal every day." I, for one, should certainly admit that, as a dressing for wounds, oil and wine are preferable to water.

Next to the simplest and most natural dressing of blood and dry lint is the method of dressing incised and lacerated wounds, even in compound fractures, with lint soaked in compound tincture of benzoin—the old Friar's balsam of well-deserved notoriety. This plan was based chiefly upon the subcutaneous principle, its object being by the exclusion of air to bring a compound fracture as nearly as possible into the same conditions as a simple fracture, but some of its advantages were undoubtedly due to the antiseptic properties of the tincture of benzoin. The late Mr. Bennion, of Oswestry, in Shropshire, adopted this plan, and obtained considerable reputation by his successful treatment of cases of compound fracture, accidents of frequent occurrence in this district. After setting the fracture, he covered the wound with a large piece of lint saturated with compound tincture of benzoin. He never disturbed the first dressing

unless urgent symptoms indicated the necessity of so doing, and if such symptoms did not appear, he would allow the first dressing to remain for a month.

[A simple method of treatment for wounds, and which owes its advantages chiefly to the subcutaneous, but in some degree to the antiseptic principle, is that of covering them with collodion. This remedy has been recently eclipsed by Dr. Richardson's new discovery of his colloid styptic.]

"The process of manufacture of the fluid," to quote from Dr. Richardson's lecture on the subject, "is tedious, but sufficiently easy. The object to be aimed at is to saturate ether entirely with tannin, and a colloidal substance, xyloidine, or gun-cotton. In the first step of the process, the tannin, rendered as pure as it can be, is treated with absolute alcohol, and is made to digest in the alcohol for several days. Then the ether, also absolute, is added until the whole of the thick alcoholic mixture is rendered quite fluid. Next the colloidal substance is put in until it ceases readily to dissolve. For the sake of its agreeable odour, a little tincture of benzoin is finally admixed. . . . .

"When the solution is brought into contact with an open surface of the body, the resultant phenomena are these: The heat of the body gradually volatilises the ether and the alcohol, and the tannin and cotton, as the ether leaves them, are thus left stranded on the surface in intimate combination. In proportion as the ether passes off, the blood or the secretion of the surface permeates the tannin and cotton; but tannin acts directly upon albumen, coagulating it, and transforming it into a kind of membrane, almost like leather. The cotton meanwhile unites the whole, gives substance to the mass, and adhesive quality. When all is solidified, the dressing becomes, in fact, a concrete, having a true organic hold or basis on the tissues; and as the tannin, if the solution be freely applied, is in excess, any new exudative matter or blood is for several hours taken up by it, and the annealing is made the more complete.

"Thus, by this dressing, the air is excluded from every possible point in every possible direction, not by a mere septum, but by the combination of the animal fluids with the remedy; and, because the air is excluded and fluid is absorbed, there is no decomposition—*i.e.*, no oxidation; and, because there is no oxidation, there is no irritation. . . .

"In cases of compound fracture, after the parts have been brought into apposition as far as is possible and fixed in the necessary position, the fluid should be poured slowly into the open cavity so as to fill it. Then the parts, externally, should

be covered with a layer of cotton-wool saturated with the solution.

"On open cancer, and on suppurating or decomposing surfaces, the solution may be freely applied with the brush, and, afterwards, the parts may be covered with cotton-wool saturated with the fluid.

"In no case need there be any fear that irritation will follow the application of the solution. On the contrary, the action of it is so purely negative that it might be considered a sedative. It is not such in the technical sense of the term, but it so effectually covers the wounded and susceptible surfaces as to maintain what is virtually a sedative influence."

I have now, for more than a year, used Dr. Richardson's colloid styptic in a large number of cases of incised and lacerated wounds, some of formidable dimensions, with complete success in a large proportion of cases. In two-thirds of the cases so treated, I can with confidence assert that union by the first intention has been obtained; or that the reparative process has proceeded either without suppuration, even in bad cases, or with the suppurative process reduced to a very insignificant amount, and in no instance have I seen any injurious effects. In about one-third or one-fourth of the cases, the styptic had to be abandoned in consequence of suppuration occurring, and antiseptic lotion relied upon.

The largest operation in which I have applied it—or rather I should say that in this case Dr. Richardson himself was kind enough to apply it—was amputation of the foot, by Chopart's operation, in a young gentleman, aged 19 years, for extreme deformity of the foot, with anchylosis, after suppurative inflammation which had attacked the left foot, and also the right knee-joint, in consequence of purulent absorption; and I should mention that he was supposed to be particularly prone to erysipelas.

In alluding to this case Dr. Richardson observes: "The operation was performed on Wednesday, February 13 of this year (1867), and as the cuboid bone and os calcis were anchylosed, the saw had to be freely used. Several vessels had to be tied, and the ligatures were left in the usual way suspended from the wound. When the lips of the wound had been brought together by wire sutures, I coated the wound freely with the fluid, and the bandage was applied. Three days later there was no fœtor, no discharge, and no general symptoms, but as Mr. Adams was anxious to see the condition of the wound I undressed it. To our delight we found it healed throughout, but, unfortunately, from the bandage adhering to one of the long ligatures, I, in removing it there, tore open the newly healed wound for the space of a quarter inch. At this broken spot

about a teaspoonful of purulent matter formed two days later; but this little break was very quickly reunited, and on the sixteenth day after the operation the patient was able to return to the country with complete healing by the first intention, and without having suffered from one symptom of a constitutional kind."

Another case was that of compound dislocation with fracture of the second phalanx of a finger, produced by a cricket ball at Lord's ground. The patient, Captain McN., came to me immediately after the accident, and after reducing the dislocation and applying three metal sutures I applied the colloid styptic and bandaged the finger to a straight splint. On the fourth day, finding no indication of inflammation, I allowed the dressing to remain, and did not remove it till six weeks after the accident, when I found the wound not only well cicatrised, but the callus was less than I expected, and some motion existed at the joint, which subsequently became quite free.

Another case was one of enchondromatous tumour, growing from the interior of the first phalanx of the third finger of the left hand of a young gentleman, Master K., upon whom I operated on April 3, 1867. My colleague, Mr. Gay, assisted me in the operation, and held the extensor tendon on one side, whilst I gouged out the growth from the interior of the bone, extending as closely as possible to the articulations at either extremity. I applied the colloid styptic to the interior of the bone, and, after applying metal sutures, painted the wound over with the styptic, and applied a little cotton wool saturated with it as a dressing. No suppuration occurred, and the wound healed completely under the first dressing, the boy recovering motion at both the articulations. Nothing could be more satisfactory.

Under my direction a large number of incised and lacerated wounds, including many of considerable size and many scalp wounds, have been treated by the application of styptic colloid at the Great Northern Hospital, by Mr. P. Hopgood, the House-Surgeon. The wounds were generally washed with ether instead of water, or with ether after water had been applied. I have before me the details of thirty-three of these cases, supplied by Mr. Hopgood, but will only read you in a few words the conclusions at which he has arrived. Mr. Hopgood observes:—"Out of the thirty-three cases of recent wounds treated with the styptic colloid, twenty healed by the first intention, requiring no further application than the first dressing; the remaining thirteen required the styptic to be removed, but still in nearly all some amount of adhesion had taken place, although suppuration had taken place in all, requiring the removal of

the dressings. The styptic colloid, in my opinion, has these advantages :—It is certainly a great improvement on the 'pad and bandage' usually used in cases particularly of scalp wounds, and without doubt favours greatly, although not certainly, adhesion by the first intention, adjusting by its contractile powers (upon drying) the surfaces of wounds together, and effectually preventing the access of air. It appears to me, from the wounds I have treated with it, to be particularly applicable to clean-cut wounds where the surfaces can be brought accurately together ; but the styptic is certainly often successful in obtaining union without suppuration in lacerated and often contused wounds, checking at all times, and often preventing suppuration in a wound which otherwise treated would be nearly sure to suppurate."

The testimony I have now given in favour of Dr. Richardson's colloid styptic as a material for the dressing of wounds, calculated to promote healing by the first intention or without suppuration, superseding the use of plasters, and embodying in the most scientific manner the antiseptic and subcutaneous principles, will, I hope, induce other surgeons to give this method a more extended trial. It is, I believe, only by such a combination of the antiseptic and subcutaneous principles that we can hope to prevent the more serious result of injuries with open wounds, such as exhaustive suppuration, diffuse inflammation, pyæmia, and death.

The methods of treatment and local applications to which I have referred, have mostly been considered in relation to their power of promoting union by the first intention in wounds of moderate size, or a reparative process without suppuration in wounds of greater magnitude. We have to deal, however, with wounds in which suppuration is already established, and with open ulcers of a more or less chronic character. To these antiseptic lotions are especially applicable. But time will not permit of my entering upon this portion of our subject, which has of late derived additional interest from the study of the antiseptic principle and antiseptic remedies.

I would merely mention that amongst the most valuable antiseptic lotions are—carbolic lotion made in the proportion of of one part of carbolic acid to thirty of water, or probably one part in a hundred would be found strong enough for ordinary purposes ; Condy's fluid, weak iodine solutions, and weak solutions of chloride of zinc, and also a lotion which has hitherto received much less attention than it deserves—viz., lime-water, as well as solutions of potash and soda. I may add that the alkaline solutions have for many years been favourites of mine, and that it has been my practice to apply lime-water constantly as a lotion to the surfaces of all ulcers and suppurating wounds ;

it cleanses the sore very quickly by chemically destroying all the pus cells and the secretion on the surface, and also diminishes the inflammatory congestion by its direct solvent action upon the fibrine of the blood in the distended capillary vessels, so that by endosmosis the condition of stasis of the red corpuscles in the capillary vessels is removed. I always use it of full strength in indolent and sloughy ulcers, in which it acts most efficiently in deodorising the ulcer, and as a slight stimulant; but in irritable sores I use it diluted with equal parts of water.

Lime-water, in combination with oil, has long been a favourite remedy in cases of burns and scalds, and in combination with calomel as black wash, and with bichloride of mercury as yellow wash, has long been considered beneficial in many cases of specific ulceration; but, as I have generally considered the benefit to result from the lime-water rather than the mercury, I have used it simply as an alkaline antiseptic lotion in this class of cases.—*Medical Times and Gazette, March 7 and* 14, 1868, *pp.* 256, 282.

38.—ON THE USE OF CARBOLIC ACID IN THE TREATMENT OF ABSCESSES.

By Dr. James Martin.

I have lately found much benefit from the use of carbolic acid in the treatment of wounds and abscesses. One case in particular forces itself on my mind as exemplifying its value.

N. I., aged 18, a stout-looking young man, but of scrofulous diathesis, presented himself to me last February with an abscess, situate to the left of the junction of the first and second bones of the sternum. It was about the size of half a large orange, and had evidently formed under the aponeurosis of the pectoral.

I made an opening at a depending point with potassa c. calce, having, for that purpose, to make a very deep eschar. The abscess discharged freely pus of a thin watery character.

I treated him locally with poultices, stimulant applications, and tents, and generally with iron, quinine, and cod-liver oil. He improved greatly in appearance, but the abscess remained unhealthy. I could introduce the probe upwards and outwards fully 5 inches, and the integuments being sound and thick, I thought it rather a formidable operation to make such a wound as laying it open to heal from the bottom would involve.

Before doing so, I happily determined to try the effect of carbolic acid. So dipping a piece of cotton, on the end of a probe, in a solution of 5 grs. to 100 of water, I mopped out all the interior of the abscess. The improvement was rapid and decided, the character of the pus improved, and day by day the

cavity contracted, until now it is at the point of being finally closed, the parts being well knit, and it being impossible to introduce the probe for the one-eighth of an inch.—*Medical Press and Circular, Dec.* 11, 1867, *p.* 530.

39.—ON THE REMOVAL OF TUMOURS BY ELECTROLYSIS.

By Dr. Julius Althaus, Physician to the London Infirmary for Epilepsy and Paralysis.

The object of this paper is briefly to state the results hitherto obtained by the method of starving tumours by electrolysis. Let me premise that the electrolytic treatment acts in a three-fold manner, viz., first, through mechanical disintegration of the tissues by the nascent hydrogen; second, through chemical destruction by free alkalies—potash, soda, and lime, which are evolved at the negative pole of the battery; and lastly, through a modification of nutrition, by the dynamic effects of the continuous galvanic current on the vasomotor nerves of the parts brought under its influence.

The entire number of cases I have up to the present time treated by electrolysis, amounts to fifty-eight, amongst which they were forty-seven non-malignant, and eleven of the malignant kind.

Eleven cases of nævus have been under my care. Of these seven were cured; one patient was only seen once, and not heard of again; one discontinued the treatment before any appreciable result was obtained; two are still under treatment, and so much improved as to give the best hopes of eventual complete success. In none of these eleven cases was there, at at any time during the progress of the treatment, any bad symptom whatsoever; and in most cases not a drop of blood was lost. In one case, the growth was so extensive that both Mr. Paget and Sir William Fergusson, who were consulted previously to the patient coming under my care, refused to interfere with the tumour. In another case, where the nævus was seated on the lower eyelid, Mr. Dixon had been previously consulted, and he advised to leave the growth alone, on account of its position, although the nævus was a source of great annoyance to the patient. Neither an awkward position, nor extreme size of a nævus, can ever be an impediment to the electrolytic treatment, which I believe to be suitable for all cases of nævus, indiscriminately.

Of bronchocele, I have had eight cases under treatment. Of these, two were cured, one was improved, five are still under treatment and progressing favourably. In several of these cases, Mr. Prescott Hewett, Mr. Paget, and Sir William Fer-

gusson, had been previously consulted, and had pronounced any of the ordinary operations to be inadmissible. Most of these cases were of an enormous size, and on that account required a long continuance of the treatment; but I believe my experience justifies me in saying that any case of bronchocele, however large, may, in course of time, be cured by eloctrolysis.

Of sebaceous tumours of the scalp and face, I have treated fourteen cases. Of these thirteen were cured; the fourteenth patient was obliged to leave town before the end of the treatment, but would from the change already caused in the tumour, have been cured by one or two more applications. In sebaceous tumours the treatment is short, as they never attain to any considerable size.

Of that hypertrophy of the skin which Mr. Wilson calls *ecphyma molluscifórme*, I have treated five cases. Two of these were seated on the upper eyelid, two on the cheek, and one on the back of the neck. Four of these cases were cured; the fifth was not heard of again after two applications, but from the change already caused in the tumour, I believe that it must have disappeared long ere this.

Of kelis, I have had one case under my care; but, as I saw the patient only once, I am unable to say whether any result has been obtained.

Of lipoma, three cases have come under my treatment. One of these is nearly cured; the other two patients discontinued the treatment before any appreciable result was obtained. One of these latter patients, a highly nervous gentleman, complained of pain and swelling of the tumour, after the application, and wrote to say that some matter was formed at one of the punctures. This is the only case where some limited suppuration seems to have taken place in consequence of the galvanic applications.

I have cured a cyst of the lower lip, in a gentleman, aged 53, and a ganglion on the wrist of a lady, aged 30, by the same method.

Of glandular tumours, I have treated three cases, all of which were much improved, but none continued the treatment until the entire removal of the enlargement. Thus of forty-seven cases of non-malignant tumours, twenty-nine were cured, ten improved, and in seven cases no result was obtained, or the result unknown.

In malignant disease, of which I have treated eleven cases, the results of the process are, as might have been expected, far less favourable. Wherever the cancer is one of considerable size, and growing rapidly, the pain may be relieved, and the further growth of the tumour be considerably checked by the electrolytic treatment; but I have not hitherto obtained a cure

of such patients. In two cases of scirrhus of the breast, where the constitution was apparently not suffering, and where the tumour was only of the size, in one of a filbert, and in the other of a nutmeg, the swelling has entirely disappeared; although, of course, I am not prepared to say that a cure has been effected. For the healing of an open cancer, the treatment seems to answer very well; I must say, however, that in no case of malignant disease have I ever trusted to electrolysis alone, but have prescribed at the same time powerful remedies to be taken internally, for modifying the constitution of the patient, such as bromide of potassium, belladonna, and quinine. Perhaps in course of time a more continuous application of the electrolytic process may be discovered, by means of which we may be able to starve out even malignant tumours with more success than has hitherto been obtained.

The only part of the electrolytic operation which is at all unpleasant to the patient, is the introduction of the needles through the skin. I therefore, always use the ether spray, unless the patient himself objects to it. To some patients the smarting of the ether is more unpleasant than the prick of the needles, but in the majority of cases, the ether spray is preferred. The galvanism itself is almost, or even entirely painless, if judiciously applied. The length of the application should vary according to the nature of the case. For small tumours two or three minutes are frequently sufficient; for large ones fifteen to thirty minutes are preferable. In the latter, the best plan is to apply the current every day for some time; but I have also obtained satisfactory results where it was only done twice or three times a week. The instrument which I have found most useful for large tumours, is the *serres-fines* conductor, made for me by Messrs. Weiss. With this, from 2 to 6 needles may be introduced singly into the depth of the tumour; their heads are then grasped by the *serres-fines*, which are connected with the negative pole of the battery by means of ramified conducting wires.

The safety of the treatment, which is its chief characteristic feature, seems to me in a great measure due to the fact of its belonging to the domain of subcutaneous surgery; for although there is a slight action on the skin, yet by far the greatest effect is produced in the substance of the tumour, owing to the superior electric conductivity of the soft and warm internal structures. Pyæmia and other much-feared consequences of surgical operations, therefore, do not follow the electrolytic applications. Add to this the painless nature of the proceeding, the absence of shock and of hemorrhage, the fact that, during the whole progress of the treatment, the patient may follow his

avocations just as usual; and I hope it will be acknowledged that this method bids fair to effect a substantial improvement in surgical therapeutics.—*British Medical Journal, Dec.* 7, 1867, *p.* 521.

40.—ON THE TREATMENT OF TUMOURS BY ELECTRICITY.

By Dr. Maurice H. Collis, Surgeon to the Meath Hospital and Co. Dublin Infirmary.

Early in the present century, when galvanism was still new, it was applied to many things, with a sanguine hope of its proving a valuable curative agent. Partly from ignorance of its nature, partly from want of experience as to its varying powers, and partly from impatience of the consequent failures, it was soon cast aside, and has been ever since, as I think, too much neglected. Its beneficial action in cases of paralysis and atrophy ought to have taught us that we possess in it an agent of great power; and the vast advances in our knowledge of its various forms and modes of action as a chemical agent ought to have led us to a more extended application of its powers as a curative agent. I must acknowledge having been led to study it in this latter respect by reading Dr. Althaus's paper on its use in the chemical decomposition of tumours. I tested his experiments, and found that there was literally no limit to the decomposing power of the continuous current derived from such batteries as Daniell's. Tumours and healthy tissues could be decomposed at a great pace—not without pain, certainly, but to any extent that might be desired, provided a battery of sufficient strength were employed. This mode of applying galvanism to tumours seemed to me very closely to resemble, as to its results, the action of other chemical decomposers, such as the strong caustics. Great immediate destruction of the morbid growth could, no doubt, be obtained, but a complete removal could not be promised, much less an immunity from relapse, or from extension of the growth by its periphery. Dr. Althaus's milder battery seems well adapted for some simple tumours, and I have seen good effects from it.

It occurred to me, however, that something could be done towards procuring the absorption of tumours, or perhaps towards checking their growth, by using galvanism simply as a stimulant. A slight occasional current, by its stimulus, will develope a wasted muscle; while the continuous use of the same current will, by over-stimulation, cause it again to waste. I therefore sought to apply this principle to tumours of various kinds, and to cause them to waste by keeping up a constant flow of electricity through them.

For reasons which I need not enter on, I tried various forms of battery; and finally I returned to the simple voltaic pile, composed of a dozen or more couples of zinc and copper an inch and a half square, or of small cylinders or plates of wood covered with felt, and wrapped round with zinc and copper wire. These simple batteries were excited by salt and water, or by sulphuric acid in the proportion of one to twenty parts of water.

The mode of application was as follows. The tumour was covered with a plate of zinc, perforated zinc, silver foil, copper, or copper plated with silver. The positive pole was connected with this plate; the negative pole with a plate of copper, which was brought into contact with the skin of the back or other convenient part. The battery, tied up in gutta-percha paper, or oiled silk, lay on a table, or was tied round the waist of the patient.

The results were as follows.

1. Complete removal of secondary cancerous deposit from a gland in the neck.
2. Rapid absorption of inflammatory deposit over and round an immense mass of strumous gland.
3. Slow removal of said gland.
4. Immediate check to the growth of a tumour composed of an aggregation of strumous glands.
5. Considerable cutaneous and subcutaneous inflammation in the same case, with softening and breaking up of the diseased glands.
6. Decided absorption of inflammatory effusions round primary scirrhus of mammary region.
7. Diminution and softening of a very firm fibrous growth attached to the periosteum.

All these results, except No. 3, were obtained in from three to six days; and all the cases had previously been submitted to a variety of treatment without benefit. Special results of a peculiar nature were observed as follows.

8. The zinc plate on the skin showed the usual tendency to decompose the skin and produce ulceration in a few hours.
9. Perforated zinc, employed to obviate this, produced phlyctenæ and pustules in the interspaces not covered by it.
10. Silver, whether as foil or plated on copper, produced redness of the skin, but with less rapid tendency to ulceration.
11. Copper plates were comparatively slow to act on skin in connexion with the negative pole; but the copper connected with the positive pole on one occasion blistered the skin severely when a very strong battery was used.

For the rest, the batteries of wire coiled on wood are much the lighter and more convenient in proportion to their strength.

They preserve their activity sufficiently, and do not wear out so soon as the voltaic pile of zinc and copper plates. They have the further advantage, that as many or as few as are desired can be used without delay in arrangement. I believe that a very strong current, whether as to quantity or tension, is not required. A small quantity is sufficient to excite the nerves of the blood-vessels; and there should be just sufficient tension to ensure that the current pass through the part to be acted on, and not merely round by the skin. I believe that, without claiming any miraculous power for electricity thus applied, we have in it an agent of considerable energy, and capable of yielding results of sufficient value to warrant us in resorting to it more frequently than hitherto.—*British Med. Journal, Dec.* 7, 1867, *p.* 521.

41.—ON TAPPING WITH THE AID OF SIPHON-POWER.

By Dr. W. Roberts, Physician to the Manchester Infirmary.

The aid which may be obtained in the operation of tapping by the use of the soliciting force of a column of water in an elastic tube was first suggested to me in a case of ascites which was under the joint care of Dr. Eason Wilkinson, Mr. Hall, of Salford, and myself, in November 1866. The disease was cirrhosis of the liver, and the necessity for tapping soon became apparent. It was agreed to try the method indicated; and Mr. Hall caused to be made a fine trocar and canula furnished with a short piece of metal pipe projecting beyond the hilt of the canula. The operation was performed in the following manner: The canula, armed with the trocar, was introduced in the usual way through the linea alba. The trocar was then withdrawn, and a piece of elastic tubing four feet long, previously filled with water, and tied in a knot at one end, was slipped over the projecting piece. The knotted end was then placed in a vessel on the floor, about thirty inches below the level of the puncture, and untied. The fluid immediately commenced to run, and continued to run for about an hour and a half, in which period several pints of ascitic fluid drained away. After this, Mr. Hall improved the instrument by adapting a screw to the projecting piece, and fixing a similar piece, also furnished with a screw, to one end of the elastic tube. The elastic tube could thus be immediately screwed on to the end of the canula on the withdrawal of the trocar, and the spilling of the fluid during the fixing of the elastic tube was almost entirely avoided.

This patient was subsequently tapped several times by the same method, and, so far as the mechanical arrangement was concerned, with cleanliness, convenience, and efficiency.

The principle of the method is easily understood. The column of liquid in the elastic tube (drawn into it beforehand by suction), acting in obedience to the law of gravity, operates as a soliciting force, and as it seeks the lower level of the vessel on the floor, it draws after it the contents of the cavity which is being tapped. When the patient lies horizontally in bed, the fluid first ascends in the canula, and then descends along the elastic tube, which thus acts after the manner of a siphon.—*Medical Times and Gazette, Jan.* 11, 1868, *p.* 34.

42.—ON THE TREATMENT OF HARELIP.

By Thomas Smith, Esq., Assistant-Surgeon to St. Bartholomew's and the Children's Hospitals.

At what age should an operation be undertaken for the cure of harelip?

The time of selection—*i.e.*, the best time, when one has the opportunity of choice—is between the third and sixth month of infant life. In recommending this period as the most favourable for the operation, I would not restrict it to any particular epoch, since many children are as robust and strong at the age of two months as others are at six. Rather consider that any age before the commencement of dentition is suitable, provided that the child is past the period of earliest infancy, is at least in fair health, and has shown by its progress since birth, and general condition, that there is a good prospect of its continued existence.

In the event of a child coming under treatment during the period of dentition, some interval between the eruption of the teeth must be selected for the performance of the operation.

It may sometimes happen that there is little choice of time for the operation—namely, in cases of newly-born infants who are unable to take the breast in consequence of the deformity, and where the performance of the operation may enable them so to do. Even in such a case, however, I would not undertake the operation unless requested by the mother. I would rather recommend that the child should be spoon-fed, either with its mother's milk or artificial food, until it had passed the tender age of earliest infancy.

I give you this advice for the following reasons :—

1st. The operation can be done more perfectly and artistically on a young child than on a newly-born infant, the parts being larger, more fleshy, and more easily handled.

2nd. A young child has, *cæteris paribus*, far more constitutional vigour and power of resistance to and recovery from injury than a newly born infant ; the younger child therefore is more likely to die under the operation.

3rd. Many infants with harelip die from causes unconnected with this deformity during the first few weeks of life, thus dispensing with the necessity of an operation.

M. Guersant, the present surgeon at the Children's Hospital, Paris, is against an early operation. Giraldès (his predecessor at the same hospital), who has had great experience in this deformity, recommends that the operation should be done in the first days of infant life, and in this opinion I am bound to tell you many coincide. The grounds of Giraldès' opinion are the following :—

1. The younger the child is the more quickly union takes place.

2. That in the case of cleft palate the union of the lip tends to bring the separated bones together, and to diminish the fissure in the hard palate.

3. That if the operation be postponed, infants get into a habit of thrusting their tongues into the hiatus between the bones, until at last the tongue acquires "*le droit de domicile*" in the palatine fissure. In a paper where this opinion is expressed, nine cases of Giraldès are quoted, and of these three were under the age of ten days when subjected to the operation; two of them died.

On this question Sir William Fergusson, than whom no one has a larger experience, thus expresses himself : "The earlier the operation is performed the better ; assuredly before teething. I decidedly prefer before the end of the first month in a healthy child, though the operation may be performed any time earlier to within a few hours of birth."

The same author, however, recommends that in the case of weakly infants, where the gap in the lip is large, and especially if the palate be cleft, the operation should be postponed for some months, until the child has acquired strength. I have myself operated on but one child under a month old. In this case union at first took place ; symptoms of syphilis, however, supervened, the line of union ulcerated, and the child died.

I have seen very many infants with harelip die before the third month, without operation, of causes not connected with their deformity.

Mr. Cooper Forster recommends the performance of an operation "at the earliest possible period, remembering, however, that loss of blood in very early infancy may be attended with the worst consequences. I should," says Mr. Forster, "have no hesitation in operating immediately after birth, so as to avoid the shock to the mother occasioned by the sight of the child. The operation has been performed successfully within seven

hours after birth. But the possibility of a fatal issue must not be forgotten."

Mr. Forster prefers to encounter the greater risk of the operation at this period, on the ground that the danger of the operation is less than that of imperfect nutrition from the deformity; and in support of this he relates two cases where death occurred, as he believes from this cause—the one at six weeks, the other at fifteen days after birth. But it is worth noticing that the death in the first case ensued from convulsions and diarrhœa the day after the operation for harelip had been performed; while in the second case the same operation had been done four days before death. Thus in both instances the operation may have had something to do with the fatal result.

The inability to suck, as far as my experience goes, is less frequently dependent on simple harelip than on fissure of the hard palate—an irremediable deformity at this tender age. In cases of harelip with cleft palate the early union of the lip certainly tends to bring the bones together; even in these cases, however, the loss of a month or two can make no great difference in this particular, especially if the mother be directed frequently to press the bones together from the first, or if the truss recommended by Sir William Fergusson be worn by the child.

There are certain imperfections in the result of the operation for harelip to which attention should be directed, since they are very likely to occur, and, when present, they seriously interfere with the completeness of the cure. These are: notching of the free margin of the lip; a dissimilarity in the horizontal plane of the opposite edges of the wound; and the occurrence of suture scars. It is to the last only that we will now allude; since, to avoid repetition, it will be convenient to consider, once for all, the different kinds of suture that are employed in the treatment of harelip.

Sutures.—Of the various kinds of suture employed for the cure of harelip, the most time-honoured and the most generally used is the twisted suture; so that in speaking of sutures the terms "twisted" and "harelip" are used as synonymous. The advantages of this kind of suture in the treatment of harelip are many and obvious: it secures firm, close, and accurate apposition; by its rigidity it keeps the exactly opposite and corresponding parts of the fissure in contact; and by the figure-of-8 thread twisted across the wound, the edges of the latter are pressed down and kept exactly on the same level. On the other hand, the following objections may be raised against its employment in certain cases—namely, that it cannot be allowed to remain in position long enough to secure the firm union of the

wound without running some risk of leaving on the lip disfiguring and permanent traces of its presence. These traces may be either a small, depressed, livid spot at the points of entrance and exit of each pin, or, together with this, a transverse bar of cicatrix across the line of the wound. I by no means wish to imply that a permanent cicatrix of the kind mentioned above is a constant result of the operation for harelip when twisted sutures are employed ; but that such an effect may occur, and is often not altogether avoidable, and that this results from the nature of the suture.

In applying this kind of suture when the edges of the wound are drawn together by tightening the thread beneath the needle-ends, the needle is raised above the level at which it was inserted, and pushed upwards towards the surface of the skin ; while the twisting of the thread beneath the needle-ends and across the line of the wound completes the circle of constriction. Thus all the soft parts included in the suture are compressed between a rigid bar of steel below, and a more or less tight and unyielding thread above—a condition which often sorely tries their vitality and powers of endurance.

This compression does not exist to any serious extent where the edges of the wound approximate easily and without tension, as in wounds of the wall of the abdomen or the face, and indeed in some cases of harelip where there is abundance of tissue; here the merits of the suture are very apparent. But it is otherwise in the treatment of bad cases of harelip in young children, where the tension of the soft parts is often considerable, and where these bear pressure badly.

The stretching of the edges of the wound in serious cases of this deformity is more considerable than in wounds generally, both on account of the width of the wound left after the operation, and because the line of the wound falls across the crown of the alveolar arch, in these cases often abnormally prominent from the projection of the intermaxillary bone.

It may be stated as generally true that where there is much tension about the edges of the wound one runs a risk of causing a permanent cicatrix, if harelip pins remain longer in the lip than forty-eight hours after the operation—too short a time to admit of the wound being firmly united.

With a view to meet these objections in cases where otherwise the twisted suture would be used, I have frequently dispensed with the thread altogether, fastening the ordinary glass-headed harelip pin by clamping a perforated shot passed over the point so as to keep the edges of the wound in contact. On many occasions I have used a suture formed of a single strand of fishing-gut, passed deeply through the wound and clamped down on either side to the skin with a perforated shot.

This has given me most excellent results, having never failed in my hands to secure union. It may be left *in situ* many days without cutting into the tissues. If there is a very scanty supply of soft parts, and much tension on the suture, it is liable to cause some eversion of the cutaneous edge of the wound; this may be remedied by tying the free ends of the gut over the wound after clamping it down on the sides. The knot across the wound may be cut on the second or third day afterwards, and the shotted portion may be allowed to remain as long as desirable.

Mr. Wood's button suture is much on the same principle, though silk is used instead of gut.

The same may be said of Mr. Pollock's metallic studs or buttons, which answer the same purpose as the shot in my suture, though perhaps more efficiently. Mr. Pollock passes a silver wire through the edges of the fissure, and fastens it on the sides by drawing it down into a slit in the stud made to receive it.

The *interrupted* suture is in every way well suited for harelip. It is used and recommended by both M. Giraldès and M. Guersant, the former of these gentlemen often allowing the threads to remain in for a fortnight or three weeks. In this kind of suture silk-thread, fishing-gut, horsehair, or silver wire may be used. Of these, the last three are to be preferred, as exciting less irritation than the first.

The *continuous* suture is admirably adapted for the treatment of harelip in every respect but one—that is, that it cannot be removed part at a time; and in this respect alone it is inferior to the interrupted suture, though for securing close and accurate contact it is to be preferred.

Whatever kind of suture be employed, it should pierce the skin a third of an inch or more from the margin of the wound, and should be passed deeply, even down to the mucous membrane, though it is well to avoid entering the mouth.—*Lancet*, *Dec.* 21, 1867, *p.* 762.

43.—TREATMENT OF HARELIP.

By Allen Duke, Esq., Dover.

Having read with considerable interest Mr. T. Smith's clinical paper on the treatment of harelip, and having considered every kind of suture recommended by him, and others to whom he alluded, with a view to prevent those ugly depressed scars and blemishes that almost invariably follow the introduction of pins and sutures of every kind *through* the skin, I cannot forbear drawing his attention to, and urging him to try, a method I have repeatedly adopted (several successful cases of which I

published in the Lancet of March 20th, 1858), as being, I think, superior to every proceeding that has been recommended. It consists in passing two or more curved needles, armed with silk sutures, from the raw edges immediately *under* the skin, carrying them through the whole thickness of the lip, and tying them *inside*. The principal advantage of this plan is that they may be allowed to remain any length of time, till the parts are firmly united.—*Lancet, Jan.* 4, 1868, *p.* 14.

ORGANS OF CIRCULATION.

44.—ON THE DIAGNOSTIC INDICATIONS AFFORDED BY BLOOD FROM THE URETHRA, RECTUM, OR STOMACH.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital.

We will suppose a patient to come to you, suffering from hemorrhage from the urethra, his other symptoms affording no conclusive evidence as to the source of the blood: have you, then, any means of finding whence it comes? I will try to answer this question for you. Firstly, urethral hemorrhage may be diagnosed by the fact that the blood precedes the urine. Should you be in doubt on this point, ask the patient to make water in your presence; if you see the blood escape before the urine, its origin must necessarily be in front of the bladder. This is so simple that I am afraid you will think I am reflecting on your rational powers in urging it; but, in spite of its simplicity, probably fifty out of a hundred medical men would not think of adopting the expedient, or would not choose to take the little necessary trouble. You next employ catheterism, and you find that the urine drawn direct from the bladder is clear (as is usual with urethral hemorrhage), or, very rarely, perhaps it may contain blood. Now comes the query—does urethral blood ever travel backwards into the bladder? I think not, or scarcely ever; since it seems so much more easy for it to run forwards to the meatus, in which direction there is nothing to oppose its escape, than for it to retrograde to the bladder, at the entrance to which is seated the firmly contracted sphincter muscle. You may start, then, with this fact—the blood from the urethra never enters the bladder; consequently blood in the bladder signifies hemorrhage from the kidney or bladder itself. Let us take some illustrations of urethral bleeding. First come those instances where blows upon the perineum are the cause. A man is riding his horse, and strikes his perineum suddenly on the pommel of the saddle, or another man falls unguardedly astride a bar of any description. Each will probably have a

little pain at the time, and, not unlikely, may vomit, as if he had struck his testicle. A little blood, we will say, comes from the penis before the patient micturates; if such an observation be accurately made, it will at once settle the diagnosis. And as regards your prognosis, it is of the utmost importance to determine if the blood does come from the urethra or from the bladder. But it may come from any part of the urethra; and to determine the site with exactitude you press with your finger on the under surface of the penis from the rectum forwards, until you arrive at a spot where tenderness exists. Take another nstance. A man jumps or falls from a great height, alights on his feet, and then falls. He at once experiences pain at the pubes, and on his going to pass water he notices blood upon his shirt, or when he has finished his micturition he sees that the chamber-vessel contains bloody urine. Now, what has here happened? The man has probably fractured the pubic portion of his pelvis; the fragments of bone have injured his urethra, and the blood is streaming into the canal at the damaged spot. But, if you have any doubt, pass a catheter, and draw off the urine. If it is clear, you may at once locate the site of injury in the urethra; and I will even go so far as to observe that with such a history the urethral bleeding is almost pathognomonic of fracture of the pubic bone. Or, one boy (A) playing with another (B) pulls him up by the collar at the nape of his neck and by his breeches behind, so as to press forcibly upon his perineum: when B comes to make water he finds some blood on his shirt, and more is running from his urethra. I am not now drawing upon my imagination, for such a case occurred in my own practice. Subsequently, the boy had abscess in the perineum, which broke and led to urinary fistula. The pressure beneath the pubes had injured the urethra at that spot. Had I seen the boy at first, I should have thought it right to pass a catheter before he made water. Having found the urine clear, and having therefore concluded that there was no hemorrhage from the bladder, I should have ordered the catheter to be worn continuously until I deemed the urethral injury repaired, in order, during this period, to transfer the urine from the bladder to the exterior without allowing it to come in contact with the hole in the urethral wall. Such is the treatment you must adopt in a like case, if you would avoid extravasation of urine and its consequences. However, this boy subsequently wore a catheter for some ti ne, during which the urinary fistula healed up, and he regained perfect health.

Violent and sudden straining, again, may induce urethral bleeding. If you see such a patient micturate, you will observe that the blood comes in front of the urine; it may be fluid or clotted, but it will assuredly come first. After over-exertion in

sexual intercourse, should blood appear at the meatus, it is most frequently from the prostate. Manipulate carefully the prostate, the under surface of the penis, and the perineum, a day or two afterwards, and a little tenderness will surely express the exact seat of injury, and of the hemorrhage.

The following are curious cases of urethral bleeding, such as are not generally recorded in systematic works on surgery. A gentleman was running up stairs to catch his child, when he felt something "give way" in the right groin, accompanied by immediate pain, and presently blood trickled from the urethra. Another man was wheeling a barrow, when his course was suddenly arrested by an obstruction. He also felt something "give way" in his right groin, and arterial blood flowed almost immediately from the urethra. In a third instance, one of our old students who was skating slipped, and suddenly felt an analogous painful sensation in his right groin; and he, like the two other patients, had hemorrhage from the urethra. In all these individual cases, being consulted, I at once passed a catheter, and in each instance found the urine from the bladder perfectly clear. According to the interpretation I adopted, the blood was the result of injury to the vas deferens; and the little artery which runs with that canal, being likewise injured, had allowed extravasation of blood to occur into the vas, and so into the urethra. Such, at least, in these instances seemed probable from the fact that a day or two afterwards each had swelling and tenderness connected with the spermatic cord within the inguinal canal. I give you this explanation as, perhaps, accounting for bleeding in some obscure cases, but I do not lay much stress upon it. (For further details of these three cases, see Holmes' "System of Surgery," vol. ii., p. 502.)

But, to digress for a short while. A patient, we will suppose, comes before you with a discharge from his urethra; it is, of course, highly important that you should know whence the pus, or the matter constituting the discharge, is derived. That it is pus may be recognised by the microscope. If its source is in the urethra, and the patient makes water in your presence, you will see that the pus comes in the front of the urine. You may then be assured that it is urethral, and comes from some part anterior to the bladder; which part is unsound, since pus cannot be derived from a perfectly healthy mucous membrane. Then, from what part of the urethra? Well, excluding gonorrhœa, you will generally find such a discharge is from the prostate, where it may be connected with prostatic calculus. But Cowper's gland and duct may be the seat of abscess, which is to be felt in the perineum; or an abscess in the walls of the urethra or deep in the perineum may cause pus to escape from the urethra. Suppose the discharge comes from the prostate, if you

pass your forefinger up the rectum, you will find tenderness when touching the prostate. Next, press upon the gland, and then with the finger of the other hand repeat the pressure along the middle line of the perineum and underneath the penis, until a little of the discharge is made to issue from the meatus, which you should inspect with the microscope. Now, although such proceedings flow from nothing but the most common-sense principles, yet it is surprising that simple expedients of this kind, by which diagnosis may be assisted and made perfect, are not more frequently chosen. I have several times made use of this method of examining a case, and nearly always with the happiest result. You do not want much of the discharged material for diagnosis. By this method I obtained from a patient, whose prostate had been inflamed and had suppurated, a small quantity of opaque milky fluid, which I took to the late Mr. Quekett, at the College of Surgeons. He examined the specimen microscopically, and at once declared it to be prostatic without being made acquainted with the history of the case. It contained microscopical prostatic calculi. A man, perhaps, comes to you saying, "I have had a little difficulty in passing my water, which is somewhat turbid; my doctor tells me that I am suffering from disease of the kidneys, and that I can never perfectly recover." The medical man has examined the urine, and, finding in it a little albumen, has jumped at the conclusion that the kidneys are at fault. But, do you examine the patient carefully; you will, perhaps, find a simple abscess in the perineum opening into the urethra, and giving rise to all the symptoms. If you gradually press the contents of the urethra forwards, you will probably find pus escape in a small quantity from the meatus.

I told you just now that blood from the urethra never travels backwards into the bladder: the exceptions are very few and special, and I believe you may safely act on that general assumption. If the third lobe of the prostate projecting into the bladder is the seat of cancerous or other disease, you may then have hemorrhage which partly enters the bladder. Again, if a catheter has tunnelled the prostate, making a false passage from the urethra into the bladder, then blood may be found in the latter cavity. Lastly, with paralysis of the bladder and urethra the latter becomes simply a dead tube for the time being; should it then be injured, there is no power of the muscle of the urethra and of the sphincter of the bladder to prevent a retrograde motion of part (at any rate) of the blood into the bladder. But, with these few exceptions, you may lay it down as a rule that urethral hemorrhage does not travel backwards to the bladder. Carry out this observation a little further. You know that extravasated blood, if it continue undisturbed in any cavity inside the body,

will in clotting assume the configuration of that cavity. And so in hemorrhage into the urethra; if the blood remains there undisturbed a little time, it forms clots which, being casts of the canal, are lengthened, and cylindrical or ovoid transversely. If there are several clots of cylindrical shape, escaping in front of the urine, you may with certainty say that they are from the urethra. But if you want to ascertain exactly their form for the purpose of diagnosis, take a little trouble about the matter, and always swim out the clots in clear cold water in a white dish or plate, giving them time to unravel themselves. If they are long and perfectly round, the site of their retention has been anterior to the prostate; if ovoid in their long axis, or like two two triangles joined together at their bases, they were moulded in the prostatic portion of the urethra, and will probably be found grooved on their under surface. Fresh blood poured out rapidly will clot along the whole length of the urethra, like macaroni. But old clots from the urethra, numerous or single, may appear cylindrical, like pancakes rolled upon themselves, until swum out in water, when they perhaps unfold; this shows a slow and limited escape of blood into the urethra.

With hemorrhage into the bladder, the form of the clots is determined by the shape of the vesical cavity. When first passed from the urethra such coagula may seem to be cylindrical, so that you are, perhaps, inclined to say they are urethral clots; but put them into a dish of water and they will then resume their flattened, circular true shape. If the bladder is full of clot, nothing but blood-stained serum and urine will, perhaps escape for days, until the clot is broken up by a catheter. Coagula formed within the bladder are somewhat irregular in outline, mostly circular flattened masses, bevelled off and fimbriated. If they have remained long *in sitû* their fringed edges are white, being formed of decolorised fibrine. Clots of this description evidently could not have formed in the urethra, nor in the ureter, nor in the pelvis of the kidney; and the blood forming them must have clotted within the bladder itself. If the clots are numerous and small, there must be many sources of the bleeding, or repeated escapes from the same source. To show you how I put these principles into practice, I will relate a case. Last Monday morning I was requested by a physician to visit a gentleman suffering from hematuria. On my entering the sick chamber three pots occupied by urine and coagula were handed to me; and I was asked to state the origin of the hemorrhage. Before examining the patient, I emptied the contents of one of the pots into a basin of water, and had the satisfaction of seeing the clots all come out with their physiognomies and faces well marked and all alike. They possessed the characters I have just described as pertaining to bladder clots.

Then came the query—what was the cause of the bleeding? Without troubling you with the history of the case, I may state that it might in that gentleman have been stone, a fungus-growth, or ulceration of the bladder. I next passed a catheter, but could find no calculus. Upon examining by the rectum, however, I detected a firm growth, in fact, a little vascular tumour, seated above and behind the prostate, and which gave rise to this troublesome hemorrhage. The diagnosis was quickly complete; but it is not too much to say that the close observation of the shape of the clots of blood principally guided me to my conclusion. Coagula in the bladder may have come down from either kidney; but if so, there would most likely be pain in the corresponding loin, at least during the passage of the clots. Besides this extra source, hemorrhage into the bladder may result from ulceration, from stone, from a small fungus growing into the bladder, from the third lobe of the prostate, from injury to the bladder caused by accidents or by catheterism, and from cancer.

Take another outlet; let us pass to the consideration of hemorrhage from the anus or rectum. A patient comes to you complaining that he is losing blood from the bowel. Your first question should be—does the blood precede, accompany, or follow, or is it mixed with, the motion? If it comes before the motion, it probably arises from the lower part of the rectum. If it comes during the motion, it is probably caused by the feculent matter running across the plugged-up mouth of a wounded vessel, and displacing the plug. If it comes after the motion, it is evidently associated with tumour or something of the sort higher up the intestine. You should then ascertain whether the blood is fresh or clotted, and if the latter, whether the clots are new or old. If it is fresh fluid blood, or bright arterial and florid, it comes from somewhere near the anus. The patient may inform you that something seems to protrude at defecation: are you to be satisfied with this? No, it is your business to ascertain the source of the blood; and as honest legitimate surgeons, you must, for the benefit of your patient, take some trouble to determine the exact nature of the malady, however disagreeable the necessary proceedings may be to you. In the case I have cited you must send your patient to the water-closet, tell him to bear down thoroughly, and examine the protruded parts, before letting him clean himself. In nine times out of ten you will see at once the source of the bleeding. The blood may be squirting on the sides of the pan of the water-closet, or on the thighs of the patient. It may be that all the hemorrhage comes from a small depressed ulcer seated near the anus, or it may come from several little raspberry-like prominences. As they are everted during defecation, it will be

extremely easy to apply a ligature or the actual cautery or nitric acid to the bleeding part, according to your own choice. I wish you, moreover, to ascertain the cause of the patient's symptoms thoroughly, because, to put the matter on a lower ground, I believe that you will not then be harassed with so much doubt and anxiety in your professional careers.

The following are two instances in which I practised the foregoing principles. A gentleman in the City, who for many months had looked exceedingly blanched and ill, came to me suffering from hemorrhage from the rectum, which had then continued for months. He was so nervous that he could not pass a motion in my presence. I accordingly ordered him an enema, and waited to see it administered. As the injection came away, the mucous membrane protruded, and an opened artery bled very freely from a punched-out ulcer, as big as a crow's quill. All this came into clear view. I ligatured the mucous membrane, including the ulcer, and the bleeding ceased from that time. He remained under treatment for weeks, whilst his health, which had been dreadfully shattered by the frequent hemorrhages, was being restored; but the bleeding never recurred, he gradually became quite well, and I now frequently see him, a strong healthy man. The wife of a surgeon who had become extremely ill from recurring intestinal hemorrhage, and who had been attended professionally during a long period without benefit, consulted me. She was even thought to have malignant disease, in consequence of her yellow appearance; and it was supposed that she suffered from cancer of the stomach or liver. I saw that the clots which passed were common spherical masses of the ordinary red colour. From this I argued that the source of the hemorrhage must be close to the anus. For, if the blood be poured into the stomach, or intestines high up, it is acted on by the gastric juice or by the acid secretion of the cæcum. Feeling sure that the origin of the bleeding must be close at hand, I passed my finger up the rectum, and felt a tender spot, which I could also distinguish to be ulcerated, owing to the different sensation I there experienced. I then introduced a cylindrical speculum, which enabled me to see an ulcer freely issuing arterial blood. I passed a sharp-pointed knife beneath the ulcer, and divided the bleeding vessel as well as the ulcer itself; from that time the hemorrhage ceased, the patient rapidly recovered, and had no return of the bleeding, and is now, fifteen years after this occurrence, in good health.

To keep our observation on the rectum. We will suppose a patient passes one large single firm clot, not several coagula; it has not, perhaps, been acted upon by the gastric juice, nor by the acid of the cæcum. The hemorrhage in such case comes

from one source, and from the large intestine not far from the outlet. The blood has rapidly escaped from the vessel, be it artery or vein ; the arterial source furnishing the firmer coagulum. But a large clot may be a collection of smaller clots aggregated. Disturb it in water, and separate the component coagula, and observe their form for diagnostic purposes. Numerous clots mean repeated hemorrhages at intervals, and in no great quantity, from one source, or from several sources at the same time. Thus you may have the same amount of clot from the rectum of each of two patients. In the one case there shall be one clot only ; in the other as many as fifty or sixty may be present, closely aggregated, so as to form but one. I had a good case illustrative of this point with the late Sir Benjamin Brodie some years ago. The patient was suffering from cancer of the rectum, and was passing masses of small coagula, which indicated repeated hemorrhage from the same spot, or from several spots. But the nature of the disease precluded us from tying any bleeding vessel or vessels. Ultimately, ice passed into the rectum arrested temporarily the bleeding. A lump of ice, I may observe, intended for such a purpose should be first placed in a warm hand, so that its sharp edges may be melted off.

If a patient vomits a large quantity of blood, the state in which it is brought up has reference to the relative quantities of blood extravasated, and of the gastric juice then in the stomach. Blood coagulates freely in the stomach, if the quantity be large, though in a peculiar manner. The free acid of the gastric fluid coagulates the albumen, and the fibrine remains fluid ; so that a stomach clot consists of defibrinated blood. The gastric fluid, moreover, converts the red colour of blood into a blackish-brown tint. If a large clot is vomited which has not been acted upon by the gastric juice, it has been caused by rapid effusion, and is probably from an artery. But blood coming from the stomach in large quantity and not coagulated evidently must be all fresh blood, and must have left the vessel immediately before coming up ; whilst, if it be grumous, nearly fluid, of a brown-bistre colour, and fragmentary, the effusion has probably been slow, and it has been acted upon by the gastric acid.

Surgeons are often accountable for the vomiting of blood, which fact you should bear in mind, or you are liable to unnecessary alarms. After operations on the lips, uvula, tonsils, or tongue, or after epistaxis, nothing is more common than for hematemesis to occur. A man perhaps receives a blow on the back of the head, and you have some suspicion, from the nervous symptoms he exhibits, that he has fractured the base of his skull. The diagnosis may be much helped if, on looking into

his mouth, you discover blood trickling down the back of his pharynx, and afterwards being vomited from the stomach. He has probably sustained a "fracture of the base," which extends across the top of his pharynx, or across the back part of the roof of his nose.

From the foregoing observations you will see that it really is quite remarkable how frequently you may employ blood for the purpose of diagnosis.—*Guy's Hospital Reports*, 1868, *p.* 22.

45.—PLUGGING THE POSTERIOR NARES IN CASES OF PERSISTENT EPISTAXIS.

By Dr. J. Ingleby Mackenzie, Surgeon to the Sidmouth Dispensary.

I was sent for to a patient, of full habit, subject to frequent attacks of rheumatic gout, who was suffering from epistaxis. Plugging the anterior nares had been adopted prior to my arrival, though ineffectually. This was repeated, the plugs being saturated with tincture of matico. Cold affusions were applied, but without success. I then proposed plugging from behind. I passed a No. 10 elastic catheter (in which I had cut an opening opposite to the usual one at its extremity) through the nostril, till I was able to seize it and draw it forward through the mouth, and passed the string to which my plug was attached through the eye made in the catheter, which, when withdrawn through the nasal orifice, enabled me, by tension on the string, to bring my plug into its required position. I fixed the end of the string to the forehead with strapping. The effect of this plugging was satisfactory; and it became necessary in three or four days to remove the plug.

I have failed to find any account of this part of the operation in any of the text-books at hand; but the plan adopted was a very simple one. I re-inserted the string in the eye of the catheter, and ran the catheter along the string up the nostril, detaching the plug, which had previously been loosened by injections of lukewarm water, caught the plug at the fauces with dressing-forceps, and simultaneously withdrew it and the catheter from the nostril.

The operation, minor as it is, is one requiring some little manual dexterity—the more especially if the patient is, as in the case to which I referred, highly nervous; which feeling was greatly increased by the sense of suffocation experienced during the operation. Bellocq's instrument is not always at hand; but there are few surgeons who do not carry an elastic catheter in their pocket-cases, and consequently have the means ready to give relief to a state which is to a patient at once distressing, if not terrifying.—*British Medical Journal*, *Feb.* 8, 1868, *p.* 115.

46.—CLOSURE OF ARTERIES BY TORSION.

By Prof. Syme, Edinburgh.

Having lately recommended the use of "torsion" on all occasions except where the great arterial trunks are concerned, I think it right to state that about a fortnight ago, having amputated the leg of a man in the hospital at the knee for disease of the joint, I twisted the popliteal artery with perfect ease and success. The stump being treated in accordance with the antiseptic system, there was no constitutional disturbance, and no suppuration. On the fourth day there was a slight serous discharge, but hardly any afterwards, and on the eighth day the wound was completely consolidated.

Although it would thus appear that arteries of the largest size are amenable to torsion, the expediency of trusting to this means for their closure, instead of employing ligatures, will admit of question until further experience ascertains how far it is safe to do so. But in the meantime there can now be hardly any hesitation in twisting all vessels of ordinary capacity; and it may be mentioned that since I adopted this system there has not been a single case of secondary hemorrhage, either in public or private.

[The proper mode of applying torsion is to twist the end of the artery until it is twisted off. We believe it is to this that Mr. Syme refers. Eds.]—*Lancet, Jan.* 4, 1868, *p.* 26.

47.—ON ACUPRESSURE.

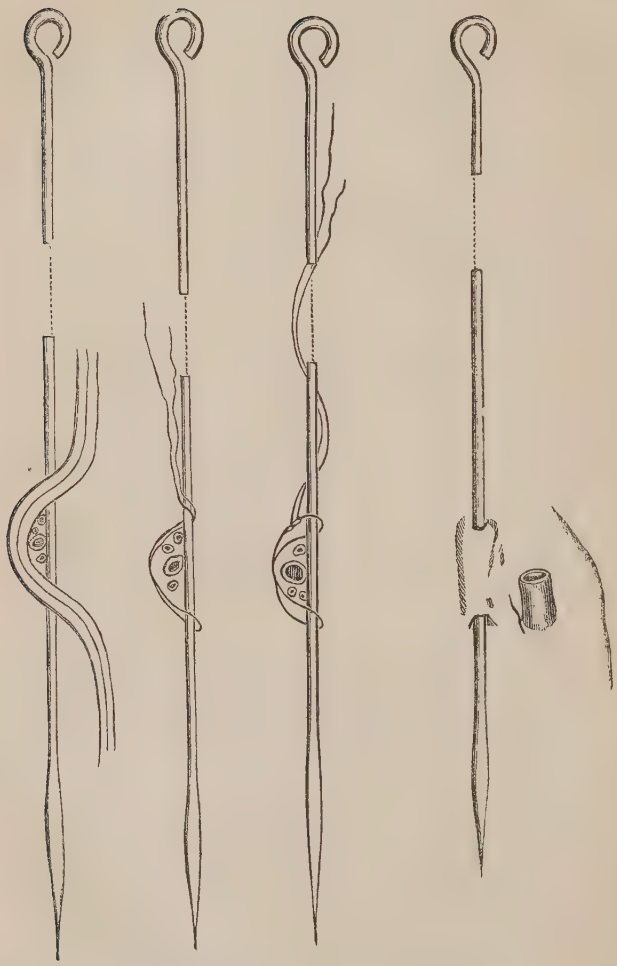
By J. Cooper Forster, Esq., Assistant Surgeon to Guy's Hospital.

[Being desirous of seeing acupressure thoroughly tried by Drs. Pirrie and Keith, and of hearing from their own lips the most successful modes of applying it, Mr. Forster undertook a journey to Aberdeen. The ease with which the needles were applied in the operations witnessed by him, together with the success attending their application, induced him on his return to London to try the plan thoroughly.]

Before detailing the cases in which acupressure has been adopted I would draw attention to the modes of procedure which I have found most advantageous. There are seven methods described in the work of Drs. Pirrie and Keith, but practically only three are constantly available; and one of these is much more useful than the others.

The methods to which I allude are the first, fourth, and fifth, and of these the fourth is the most useful. Let me now proceed to describe these plans, for I feel assured that if acupressure

Fig. 1. Fig. 2. Fig. 3. Fig. 4.



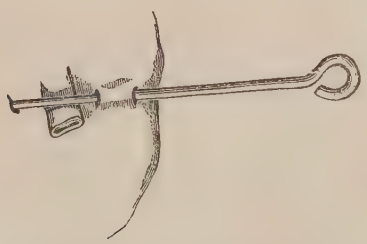
be generally adopted (as I sincerely trust it may be), one or other of them will be pursued.

The first method consists in passing the pin from without through the skin, by one side of, and close to, the bleeding vessel, and pushing the point through the skin so that the artery lies acupressed between the pin and the skin. This is shown in fig. 1 in the plate. Where the vessel runs superficially it is a very satisfactory mode of acupressure : it is not, however, thought to be applicable to a large vessel, nor to one that requires much pressure—not being a very secure plan : still the vessels of the cord are amenable to it, and the arteries of the forearm.

The fourth method consists in passing the pin underneath the vessel in the wound, slipping a loop of wire over the point of the pin, and compressing the bleeding vessel between the double wire and the pin, fixing the wire tightly by coiling it around the pin again. The vessel appears as shown in fig. 2.

The fifth method (known as the Aberdeen twist) is practised without a wire, as the first, and consists in passing the pin by the side of the bleeding vessel, bringing the point out of the flesh, then turning the pin round a quarter of a circle, allowing its point to go over the vessel, and fixing the needle into the soft part beyond. It is difficult to show this in drawing, but figs. 4 and 5 will give a tolerably fair idea of the disposition of

Fig. 5.



the pin in the two stages : first, that of introduction of the pin, and secondly, that of fixation. It suffices to say that the vessel is compressed between the pin and the soft parts ; these methods are demonstrated in a few minutes by any one who has adopted the plan, and rendered perfectly intelligible, but I doubt whether any description, however accurate, can render them so plain as to make them available by any one who has not seen the process.

The plan suggested by Mr. Durham consists in passing the pin as in the fourth method, and in using well waxed silk ligature or fishing gut, which is better, instead of wire; this is looped around the point of the pin, crossed over the artery, soft parts, and pin, and tied underneath the latter on the opposite or head end of the pin; the vessel is thus compressed between the pin and the doubled silk or gut. This is seen in fig. 3.

[Mr. Forster then relates numerous cases in which these methods were used, and proceeds to say:]

Acupressure, to be satisfactorily performed, requires considerable practical experience. There is no doubt that the exact amount of pressure required to compress the vessels can only be learnt by practice (in two of the cases herein detailed hemorrhage occurred, apparently from want of that proper amount of pressure); hence there will always be some difficulty in instructing students and others in the plan, especially in its fourth method, the most useful. In some cases it appears that very little pressure, properly applied, will restrain the bleeding even from a large artery, as witnessed in the fifth method; in others, if the vessel be not exactly hit, or there is more muscle than there should be between the wire and the artery, or if more tissue intervenes between the artery and the pin than is desirable, the wire must be tightened with considerable force ere the hemorrhage is controlled. Should there, however, be any difficulty in arresting the bleeding from these causes, the pin can easily be taken out and re-introduced closer to the vessel, or a fresh wire applied. A little practice soon overcomes these obstacles.

By many it is said, "We do not doubt the possibility of restraining hemorrhage by this means, but in what respect is it superior to the ligature?"

Some reply, "In the avoidance of small sloughs." We believe much more importance than they deserve is attached to these small sloughs, which are said to be caused by the ligature strangulating the part of the vessel beyond the spot where the thread is tied; they (the sloughs) are said to be the cause of pyæmia, suppuration, and other evils; be it so, but the piece of tissue which is compressed between the pin and the wire has its vitality very materially damaged, if not destroyed. by the compression made use of and though, as a rule, forty-eight hours is the longest time (and less than this is frequently allowed) for the pin to remain, I believe it is sufficiently long to seriously damage the life of the part beyond the pin and wire, and thus there is no particular advantage gained by acupressure on this score. Much remains to be known regarding the length of time

during which the pin should be allowed to remain before the artery is secured, but whatever time elapses, the part must be squeezed rather severely between the pin and wire, if the latter be used.

Other objections are raised by inquiring what becomes of the accompanying nerves and veins when acupressure is employed: are they not seriously damaged?

In the case of the veins I can only say it is a great advantage to have their disposition to bleed arrested in so simple and efficient a manner. No damage seems to result from their compression, nor from compression of the nerves. I have not heard of any expression of pain at all greater than is common after amputation, when the ordinary ligature has been employed, and when the greatest care has been taken not to tie the slightest filament of a nerve; on this head therefore I believe the advantage to be considerably on the side of acupressure.

The records of Drs. Keith and Pirrie as regards pyæmia are very satisfactory, but that may be accidental. In the cases I have reported there have been four deaths from pyæmia after amputations—two of the thigh and two of the leg; but in justice it should be observed, that in one of these cases the patient was unfortunately in the next bed to a man in whom erysipelas appeared; in the other case, a double amputation was performed, the vessels being secured by ligatures in one leg, and by acupressure in the other; in another there was phlebitis previous to the operation. There remains then only one case (8) in which acupressure was employed, and, without any exceptional occurrence, pyæmia took place.

One great advantage promised by the advocates of acupressure is, that wounds in future will unite by adhesion, where now they heal by suppuration. On looking over the work of Drs. Pirrie and Keith before alluded to one is dazzled by the success they have obtained, and therein reported; it is totally at variance with my experience in the thirty-one cases I have recorded; it must be considered as exceptional. The sanitary condition of the Aberdeen Hospital, and the arrangement of the beds there, certainly did not strike me as peculiarly favourable to the adhesion of wounds. On the contrary, I look upon the scene of the labours of those surgeons as one that must frequently be the source of great annoyance to them, as likely to retard the rapid convalescence of their patients. I need only mention the fact that on one side of the wards there are no windows, nor any means of ventilation, except by doors opening into a dark passage, which runs along the centre of the building (a particularly objectionable style of building, to my mind). We must, therefore, look to other causes for the wonderful plastic power from which such marvellous effects

have resulted in the far north, and attribute it to the excellent health of the patients on admission, or to their peculiar diet—the porridge, Aberdeen bap and Finnan haddock indulged in for breakfast, with goodly potations of the favourite stimulant of the country, or to the fresh air and sea breezes of Aberdeen, and not to acupressure alone: otherwise some measure of similar success might have fallen to Mr. Durham and myself. We have been most careful to arrest the hemorrhage from all the bleeding vessels, and to get the wounds to a certain extent glazed before bringing the edges together, yet, notwithstanding all the pains taken, perfect primary adhesion, without a drop of pus, has not in one instance rewarded our care thus far.

Regarding the probability of secondary hemorrhage after operations, of course it may occur both when vessels have been acupressed and when the bleeding has been arrested by any other means, but owing to the possibility of including several vessels at once in acupressure, it is less likely to take place than when a ligature is applied round the special vessel; and here acupressure undoubtedly becomes a great gain. That a smaller number of foreign bodies need be employed in acupressure than when the ligature is used, is evident from the twenty-first case, where but one pin was required to arrest the bleeding vessels of the arm in a child, where I was compelled to amputate; this could scarcely have been the case if ligatures had been employed.

What great advantages, then, are offered by acupressure in securing bleeding vessels where adhesion of the surfaces of the wound is required? for, be it distinctly understood, where no adhesion is required, there is no necessity for acupressure.

1st. The ease with which one person can arrest bleeding. The surgeon is independent of the assistance of any one to tie the vessels while he is taking them up with the forceps: this is by no means to be despised in country practice, where assistants are not as plentiful as in a London hospital; perhaps the spring artery forceps may be considered by some as affording an equal advantage; I do not share that opinion.

2nd. The control of the venous hemorrhage at the same time with the artery. This could, of course, be accomplished by tying both artery and vein in one ligature, a plan certainly not to be recommended, although I never shared the fear that was formerly and is now sometimes expressed, of tying a vein; still it appears rather a clumsy way of arresting the bleeding of the two, to put one ligature around them, when a pin and wire for twenty-four hours will answer the same purpose.

3rd. The short time (forty-eight hours at the very longest that a pin, or pin and wire, if both be used, need be left in the wound, constitutes to our minds one of the greatest advantages of acupressure; I believe that only those who have tried the

plan, amid the obstacles which invariably present themselves to the adoption of any new course, can appreciate the satisfaction felt by the surgeon who (in an amputation of the breast for instance) can, at the end of less than twenty-four hours, remove from the interior of the wound all foreign bodies; not alone by the surgeon is the satisfaction felt, for I have observed the patient to rejoice at the absence of a something which she knows has been placed there for her safety, and has so speedily fulfilled its purpose. The surgeon can also honestly state that there is no obstacle, in the shape of any extraneous body, to the entire union of the wound after the pins have been removed.

There is a satisfaction about this removal of the pins, which, I repeat, can only be felt by those who have confidence in the proceeding, and who will give it a fair trial (as I have done). This alone would induce me to use it as a means of arresting hemorrhage in preference to the ligature.

4th. How often has the surgeon for several days enjoined especial care upon the dresser, and used the same himself, lest any untoward or accidental pull upon the ligatures should tear them away before the perfect securing of the vessel has been effected. All this caution is rendered unnecessary, or, at any rate, exists but for a few hours where acupressure is used.

5th. The offensive moist seton, in the shape of the ligature hanging from the wound, is gone; is not this in itself sufficient to recommend the use of the pin and wire? I think so, and have no hesitation in advising those to try it who at present condemn it as not being sufficiently advantageous to warrant a departure from the old custom.

I believe it to be necessary that acupressure should be seen carried out by some one tolerably perfect in it before it can be sufficiently understood to be practised; no mere verbal description will suffice. It was the inadequacy of description which induced me to see the plan in its entirety at Aberdeen, where undoubtedly (at least so far as I can understand) it is more generally in use than elsewhere. To my mind the difficulty of describing the plan is sufficient to account for the failures of which I have heard, and the deaths from hemorrhage put down to its use, which have unquestionably shaken the faith of more than one person in its efficacy as an hæmostatic agent.

It should never be forgotten that acupressure need only be employed where adhesion of the surfaces of the wound is required; nevertheless it may be used where any hemorrhage is taking place, if the vessel can be acupressed without much difficulty, and also when speedy withdrawal of a foreign body is advisable. In secondary hemorrhage from the palm of the hand (as in Case 22, where I never saw anything more satisfactory) the usual practice would undoubtedly have been to have tied both ulnar

and radial arteries, or, perhaps, the brachial, instead of which a pin was placed under the two former, and removed in forty-four hours, with the most perfect control of the hemorrhage, which did not again occur. It was quite impossible to secure the vessel at the seat of injury, as I believe the bleeding came from the deep palmar arch.

What are the disadvantages of acupressure? Suppuration along the track of the pin certainly may occur, but it is very rare. It is asserted by some of its advocates that so great is the tolerance of the flesh for metals, that foreign bodies of that nature may lie for weeks, nay months, or even years, in the body, without giving rise to anything more than pain, and that, therefore, a pin will not cause suppuration along its track. One fact is worth a dozen conjectures. In Case No. 17 before mentioned it will be observed that considerable suppuration occurred along the course of the pin, and I may notice that the irritation did not subside for many days, yet the pin was only kept in for forty-eight hours—too long a time I quite admit, and which I should carefully avoid allowing to elapse before removal of the pin on another occasion.

Another objection sometimes raised is the protrusion of the pins and wire from the wound, giving the stump, when many pins are used, the appearance of a well-stocked pin-cushion. So frivolous an objection can hardly be entertained seriously, but, even were it worth considering, as the disagreeable condition lasts, at the longest, but forty-eight hours, the objection is, I think, of no practical importance.

To a patient the presence of the sharp points of the pins, if the first method be used, is objectionable, on account of their catching in the bedclothes; but, of course, the points can easily be protected.

It is said by some opponents of acupressure that the pain on the removal of the wire is a serious obstacle to its use, and undoubtedly some little irritation is very frequently felt when the pin is being withdrawn. At the subsequent withdrawal of the wire there is a momentary pain, but that does not always occur; still I will admit there is some objection to the plan on this score, but this is not the case when Mr. Durham's mode is adopted, as the soft silk or gut slips off more easily than the wire, and any one who gives these materials a trial will, I think, be very satisfied with them for this reason amongst others.

How acupressure acts in arresting the course of the blood in a vessel so completely as that in a few hours the pin may occasionally be withdrawn, is, I confess, a difficult matter to explain: and it is not the object of the present paper to discuss this question, but rather to assist in advocating the use of acupressure, instead of the ligatures, not by endeavouring to prove it a

wonderful means of obviating all the dangers attending operative surgery, and of getting adhesion in wounds which would never otherwise unite; but rather by stating fairly, and without prejudice, what it does and what it does not do. For my own part I have quite decided in its favour (for the reasons I have given), and never intend again to use a ligature in any case where the edges of a wound are brought in apposition with a view to their adhesion. That some more simple plan than that of the pin and wire may be substituted I fully believe, and shall be most happy to cast them aside, as I now do the ligature, when such contrivances shall have been suggested; until that time arrives I shall continue to use acupressure as at present applied, in lieu of the ordinary ligature, with the same feeling that prompted the use of that means of arresting hemorrhage, instead of the hot iron, for I consider it as great an advance on the ligature as the ligature on the cautery.—*Guy's Hospital Reports*, 1868, *p.* 113.

48.—THE CLOSING OF BLEEDING BLOOD-VESSELS BY ARTERIAL FORCEPS

By Dr. William Taylor, Edinburgh.

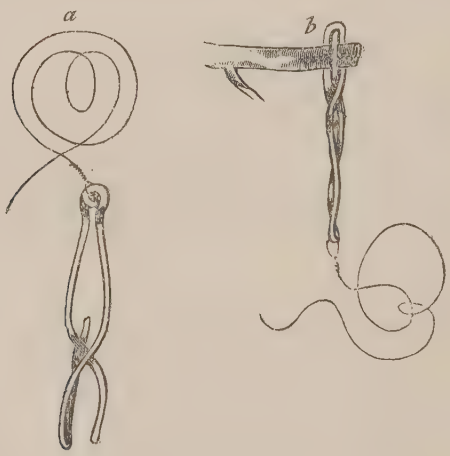
[The following article was elicited by that of Mr. Nunneley, of Leeds, given in our volume for last half-year. The experiments recorded in Mr. Nunneley's paper bear a close resemblance to a series in which Dr. Taylor was engaged when a pupil of Sir James Simpson two years ago. These were embodied in a thesis presented to the medical faculty of the University of Edinburgh, in March 1866. The following is an extract from this thesis.]

This may be regarded as emphatically the iron age in surgery. The knife has, in a great measure, supplanted the caustic holder; the metallic suture, the silken seam; and now acupressure threatens the extinction of the ligature. We cannot have too many changes, provided that each be an improvement on that which went before; and, as acupressure is manifestly a step in the right direction, let us lend our aid in bringing it as near to perfection as possible. A healthy cicatrix is a far prettier sight than a suppurating sore, and a week's superintendance far preferable to a month's dressing; and if such benefit to our patient and such pleasure to ourselves can by any means be obtained, even once out of a hundred times, I hold it to be our duty to give him the full benefit of that chance. But the advocates of acupressure expect—and I think reasonably—far higher triumphs than these; they expect to see at least fifty per cent. of their patients benefited by its use; and they have the pleas-

ing reflection that those wounds which fail to heal by first intention under its application, belong to a class the healing of which is not by any means favoured by the ligature.

Although convinced of the supraexcellence of acupressure, I confess that, like many others, I have failed to see the possibility of its universal application, more especially to the deeper seated parts ; and, although firmly believing that the ligature is most certainly doomed to disappear from our vision within half a dozen years, still I think it probable that something besides the straight needle, but made to act on the same principle, will be required to aid in supplanting it.

Acting under these convictions, I set myself to devise a method whereby all the advantages of acupressure might be applied not only in regions where direct pressure from a straight needle would be unattainable, but also in parts that are fairly accesible. The result has been the constrnction of a self-acting wire-compress, which I have dignified with the name of "acupressor arteriæ." This minute piece of mechanism is so constructed as



a. The acupressor arteriæ placed in readiness to grasp the artery.
b. The acupressor arteriæ closed upon the artery.

to retain a firm hold of the artery, and, although hid from view by being enclosed within the divided tissues, it can be removed at pleasure any time after the operation. It is therefore tantamount to a metallic ligature, for the separation of

which we have not to wait for two, three, or four weeks, but which can be removed at any moment without the slightest trouble. It consists of a needle, or rather a piece of steel wire, bent upon itself in the middle and turned into a single spiral, which constitutes a spring; the extremities are so adjusted that by the aid of the spring, they lay hold of the artery to be compressed. The compress is then left in the wound, but connected with the spiral at the end is a small wire; this is allowed to hang out in the same manner as the ligature is at present. When the time has come for its removal, a slender silver tube is passed inwards over the wire until it reaches the acupressor arteriæ; the wire is then held firmly by the left hand, and the tube passed steadily inwards with the right. On reaching the converging arms of the instrument, they become compressed within it; the extremities then relax their grasp of the vessel, and the whole is gently withdrawn. (See illustration). Its advantages are are numerous.

1. It is perfectly secure. It will be noticed that, when in use, the reflected limb acts as a spring, retaining the other in its place.

2. It is easily applicable. Anybody could apply it with security, whereas often a well-tied ligature may give way.

3. In cases of necessity, it may be applied without the aid of an assistant, and would, therefore be invaluable alike to the military surgeon and the country practitioner.

4. It can be applied in one-third of the time it takes to put on a ligature, and in any part where the ligature can be applied.

5. Its great simplicity and cheapness are a recommendation.

6. It is applicable to the treatment of aneurism. It may be applied at the Hunterian site; and in a few days will cause partial or complete occlusion; after which it may at once be removed.

7. It is perfectly applicable to deeply seated parts; wherever the knife goes, it can follow, even although the wound be small. For this purpose, it should be caused to gape by being partially drawn within the end of the tube; it is then applied by being pressed outwards from the interior of the tube, and the tube withdrawn.

I hope that soon all these statements respecting it may be confirmed, and that it may be found useful to the profession.

Forceps of the same kind and for the same purpose as those described by Mr. Nunneley, have, I know, been suggested by Dr. Hoffman, and Mr. Staunton; but such instruments are far too cumbersome to admit of their being a benefit to surgery; while the cost would be so great as to prevent them from ever being universally adopted. The acupressor arteriæ, on the other hand, is simple in construction, and could be manufactured

by the gross at a very small cost. It will, I think, be found exactly to supply the defect which Mr. Nunneley confessedly feels in the use of his forceps, and of which he is now endeavouring to discover the remedy. It is, in fact, the realisation of what he aims at when he says: "I think it probable that manufacturers of the safety-pin, or like handicraft workmen, may be able to supply a forceps sufficient for all purposes, at so small a price as to allow it to be used without any consideration of economy. This I am taking steps to ascertain. At one time, I thought of employing forceps whose blades are brought together by a screw motion or by a sliding tube; the latter idea had also occurred to Mr. Birkett," &c.

q *p*

In the case of the acupressor arteriæ, the sliding tube is made use of for the liberation of the artery, or in cases where the instrument has to be applied to an otherwise inaccessible region. It forms no part of the instrument, but may be used for any number of them; thus the objection of costliness is obviated.

Mr. Nunneley says further: "I think it by no means improbable that, in some cases of aneurism, compression by similar forceps, of the artery, on some point of its course above the sac, for a greater or less length of time, may be successful. The sheath of the vessels should be merely punctured to an extent sufficient to allow of the forceps being glided upon the artery alone, no other part being included or disturbed," &c.

It will be seen, by the extract given above, that these ideas with regard to aneurism had also been anticipated in my thesis.—*British Medical Journal, Feb.* 1, 1868, *p.* 92.

49.—ON A NEW METHOD OF ARRESTING HEMORRHAGE BY CANULA ARTERY FORCEPS.

By Dr. J. R. Wolfe, Ophthalmic Surgeon to the Aberdeen Royal Infirmary.

[Dr. Wolfe, who certainly at Aberdeen must have seen a good deal of acupressure, considers the several pins and loops of wire used in that process too cumbersome and formidable to recommend themselves to general use.]

Being in the habit of using extensively the *serres-fines*, and having daily opportunities of witnessing their superiority even over the metallic ligature, the idea suggested itself to me of applying the same to the securing of arteries. The *serres-fine* is exactly the self-acting forceps of Mr. Nunneley, published in this Journal of October 12th. But, owing to the difficulties I experienced in withdrawing such an instrument from a deep wound, and to the danger of leaving it in an artery unguarded I planned an instrument, which Mr. W. B. Hilliard of Glasgow constructed for me, and which I call the "canula artery forceps." It is so exceedingly simple, that the annexed drawing almost explains itself. A, shows the instrument open; and B shows the same closed. The sliding tube serves the purpose of a clamp. The largest does not exceed the size of a large acupressure-pin. Smaller sizes are used, according to the depth of the wound, or the size of the vessel to be secured. The cost is only one shilling each.—*British Med. Journal, Nov.* 16, 1867, *p.* 444.

ALIMENTARY CANAL.

50.—POSITION IN THE REDUCTION OF INGUINAL HERNIA.

By Dr. Joseph B. Bond, Yarmouth, Nova Scotia.

Several years ago, I had a case in which the patient could not reduce an inguinal hernia while lying in bed either on his side or his back, but as soon as he stood on his feet there was not the least difficulty. If, on removing his truss before going to bed, he neglected to apply his hand to the part and allowed the rupture to protrude, he had always to get up on his feet before he could reduce it. Soon after this I was called to a case of strangulated inguinal hernia. After making every effort in the usual way to reduce it, I directed the patient to stand up; I placed myself (also standing) behind him, and encircled his body with both my arms, grasped the tumour with both hands, and effected in a few minutes what I had failed to accomplish in as many hours. Since then I have had many cases of inguinal hernia in my own practice, and several where I have been called in consultation, and have never failed to effect a reduction in a few minutes in the way I have described. I have never seen this means tried in the Hospital in Philadelphia nor in the London Hospital, although in both these institutions I have repeatedly seen all efforts fail to reduce an inguinal hernia without an operation. Nor have I ever seen it recommended in any surgical work.

My object in sending you this communication is to ask my medical brethren of the metropolis to give the erect posture in

the reduction of inguinal hernia fair trial, and to publish the results. In femoral hernia the erect posture has never succeeded in my hands—in three cases I have been obliged to use the knife—in inguinal never. I will not attempt to account for the use of the erect posture in the reduction of inguinal hernia, nor for its failure in femoral. It may be thought that the erect posture favours reduction by causing syncope, but in only two cases do I remember that a feeling of faintness was complained of. In the last case (only a few days ago) the patient, an old man, fainted and fell as soon as the gurgling began to be felt, and I finished the reduction whilst he was prostrate.—*Medical Times and Gazette, Jan.* 4, 1868, *p.* 23.

51.—ON THE TREATMENT OF OBSTINATE CONSTIPATION BY FARADISATION OF THE BOWEL.

By Dr. Julius Althaus, Physician to the London Infirmary for Epilepsy and Paralysis.

[In the following paper it is the object of the writer to place on record a few of the numerous cases of obstinate constipation which he has found to yield readily to faradisation of the bowel, after the failure of other modes of treatment. The first case is that of a lady suffering from headache and other symptoms arising from habitual constipation, which Dr. Althaus had not succeeded in relieving by the use of ordinary purgatives.]

I now proposed to the patient the application of galvanism, for inducing a healthier action of the bowels; and, as she consented at once, I introduced an insulated metallic sound, the top of which only was free for about an inch, and which at the other extremity was connected with the negative pole of the primary faradic current, into the rectum, a moistened conductor connected with the positive pole being placed on the abdomen. A current of moderate power was then allowed to pass for fifteen minutes. The patient did not complain of any pain or discomfort from the application, but said she felt more lively and hopeful than she had done for a long time.

24th. She came to me in high spirits, saying that since the faradisation was used she had every day had a better motion than for many months past. I ordered her now to discontinue the colocynth pill entirely, and merely to take a wineglassful of Friedrichshall water thrice a day.

From that time forward the patient made an uninterrupted recovery. Faradisation was continued twice a week for a month, after which neither medicines nor mineral waters nor the galvanic stimulus were any longer required. The head symptoms disappeared gradually, in proportion as the action of the bowels

was restored ; and when I last saw the patient (July 1867), she was perfectly well, excepting the somewhat too copious menstruation, which was now the only trouble she experienced from the fibroid tumour of the uterus.

Case 2.—A married woman, aged 28, came to me in July 1866, with the complaint that her bowels were very much bound, and that she suffered from difficulty of walking and micturition, and a sense of distressing fulness in the pelvis. She said that her parents had made her take Morison's pills from a very early age for everything that ailed her, whether it might be a headache, a bad finger, or a sore throat. She had always had difficulty with the bowels after taking pills for some time. She was married in 1863, and had had two children, the last in January 1866. After the first confinement, the bowels were very sluggish indeed, and required strong aperients to make them act. After the second delivery, this condition was, if possible, worse. She now went sometimes a whole week unrelieved, and, on making exertions for defecation, experienced intense pain about the diaphragm, so as to feel quite sick and giddy. She was restless at night, and generally thirsty, but the appetite was very feeble. The tongue was dry, the abdomen very full and tense, and painful on pressure. Pulse 90. On examining the rectum, I found it choked with hardened fæcal matters, some of which I scooped out at once. I then inserted the negative electrode of the primary current, as above, and sent the galvanic stream through the bowels for ten minutes. On the evening of the same day, a very large quantity of highly offensive fæcal matters came away, with a feeling of intense relief to the patient. Suitable rules as to diet and regimen having been given, and faradisation having been repeated four times more, the patient could be discharged cured. I saw her in May last, when she told me that she had continued perfectly well ever since.

Case 3.—A hard-working merchant, aged 38, first consulted me in June 1865, for a troublesome form of dyspepsia, from which he had suffered for many years past. He complained of a feeling of heaviness and oppression after meals, especially after breakfast, coupled with eructations and flatus, which latter gave rise to acute pain until they were discharged. The bowels had been habitually costive since a journey the patient had undertaken to the west coast of Africa, in 1857, where he had suffered from a bad form of dysentery. Ever since that time he had been obliged to "assist the bowels" with purgatives. He had also used enemata of various kinds, but the rectum seemed to resent them, and the patient had taken a great dislike to their use. The tongue was dry and furred. The patient had lost flesh lately in consequence of being compelled to restrict

his food to the smallest possible quantity. The urine was clear; the motions dark, hard, and ill-formed.

I carefully regulated the diet, and prescribed a tumblerful of Eger water night and morning, and five grains of carbonate of bismuth twice a day after meals. Digestion and defecation soon improved under this treatment, so that the patient was able to take more food than before, and he felt and looked much healthier and stronger than he had done for a considerable time past. About two months after I first saw him, business obliged him to return to Africa, and I did not see him again till March 1866, when he was very much worse than he had ever been before. He had been obliged in the interval to take strong purgatives habitually, for ensuring sufficient action of the bowels. Indigestion and costiveness were now more troublesome than ever. The patient was emaciated and exhausted to the last degree. He was unable to do any work or take any exercise, and refused all nourishing food, so that his family were extremely alarmed about his condition. Under these circumstances, an energetically tonic plan of treatment appeared indispensable. I prescribed liquor arsenicalis, with vinum ferri, a dose of pancreatic emulsion in rum and milk twice a day, a compound rhubarb pill, with the twenty-fourth part of a grain of strychnia at bedtime, and faradisation of the bowel twice a week. Under the influence of this treatment the patient rallied wonderfully. The beneficial effects of faradisation in inducing a healthy action of the muscular coat of the bowel were well shown by the circumstance that there was always a very good motion the evening after the application of the electricity, while on those days where it was not applied the action was rather sluggish. At the end a month the aperient pill was discontinued; the arsenic was taken for another fortnight, and the emulsion for a month more. Faradisation was discontinued at the end of three months, when digestion and defecation were normal. Ever since that time the patient has been in good health, and taken no medicine whatever.—*Lancet*, *Nov.* 16, 1867, *p.* 607.

52.—FLATULENT DISTENSION OF THE COLON, WITH OR WITHOUT DIARRHŒA.

[We take the following short extract from a paper by Dr. HABERSHON, of Guy's Hospital, on "Functional Disease of the Colon."]

To check the flatulent distension of the colon from gaseous evolution, creasote and carbolic acid hold a deservedly prominent position; and when there is also diarrhœa, *carbolate of lime* may be given internally with marked benefit. The carbolate of lime

as prepared by Mr. Squire, of Oxford-street, is perfectly white, and the smell is not so strong as the ordinary carbolate of lime used as a disinfecting agent. I have generally given it in one-grain doses combined with henbane ; and I may remark that in the diarrhœa of phthisis, where there is evidence of fermentative changes in the colon, I have repeatedly used the carbolate of lime with good effect.—*Lancet, Jan.* 4, 1868, *p.* 8.

53.—DISEASE OF THE GUMS.

By — Hilditch, Esq., Brighton.

The teeth should be washed night and morning, a moderately small and soft brush being used ; after the morning ablution, pour on a second tooth-brush, slightly damped, a little of the following lotion, and apply it to the affected parts : Carbolic acid, one scruple ; rectified spirits of wine, two drachms ; distilled water, six ounces. Lister's brilliant inductions gave me the idea of using carbolic acid in these cases ; by it suppurative action is kept under, and the gums soon get firmer and less tender.—*Lancet, March* 14, 1868, *p.* 365.

ORGANS OF URINE AND GENERATION.

54.—ON URINARY DISEASES.

By Sir Henry Thompson, Surgeon to University College Hospital.

[In forming a diagnosis in a case of urinary disease, and this indeed applies to most other diseases, we must adopt a pretty uniform mode of interrogating the patient. Our object is to collect *facts*, and our diagnosis consists of the inference which we draw from these facts.]

First of all with regard to questions. You may make out most cases of urinary disease—say five out of six—by four simple questions, including the minor extensions which arise out of them. I always ask the patient these four questions, and in the following order. The first is, "Have you any, and if any what, frequency in passing water?" Then, as a branch of that question, springing out of it, I ask whether the frequency is more by day or by night, or influenced by any particular circumstances. How the question applies I will tell you afterwards. Then, secondly, I ask whether there is pain in passing urine, and whether *before*, or *after*, or *during* micturition ; and whether at other times also, and if produced or aggravated by quick

movements of the body. The locality of the pain is also to be ascertained. Then I ask as a third question, "Is the character of the urine altered in appearance? Is it turbid or clear?" Possibly the patient will tell you that it is turbid; but you find, on questioning further, that it was passed perfectly clear, and only became thick after cooling or standing. The fourth and last question is—whether blood has passed in any way, whether it is florid or dark, whether passed at the end or at the beginning of making water, or whether independently of micturition altogether; and such-like supplementary inquiries. These are the four questions, and let me remark that the answers you get will depend very much upon the way in which you put the questions. The patient is not always self-possessed, or he does not clearly understand the nature of the question you put. It is necessary to be very precise and very distinct in your questions if you wish to get accurate answers. In fact, there is no such difficult thing in all experience, whether in our profession or out of it, as to arrive at facts; and let me remind you, again, that diagnosis consists of the acquisition of *facts*, and that it is impossible without them. Now you will say, how do I apply this to the list of diseases before you.?

First, as to the frequency of passing water. There is no serious affection of the urinary organs, except one or two which I will name, in which you have not more or less frequency of passing water. For example: A man may have stricture to a considerable extent; the stream may be very narrow, and he may not for some years complain of frequency of passing water. I have classified these diseases, as you see, so that we may deal with them more easily. First of all, there are the inflammatory diseases—inflammation of the urethra, of the prostate, and of the bladder. In all these you have frequency in passing water. Not necessarily, however, in urethritis, until it reaches the distant part of the canal near the bladder. I do not propose to enter upon the subject of urethritis here, as you have frequent opportunities of studying it in the out-patient's room. I am now only referring to this symptom of frequency of passing water as existing more or less in all these three diseases at some time or another. In hypertrophy of the prostate you have it, and it is remarkable that it is more at night than in the day. In chronic prostatitis it is usually present to a small extent; in cystitis it is, of course, a characteristic symptom. In calculous diseases it is prominently met with, and generally its degree is in proportion to the amount of movement permitted to the patient. Tumours, of course, malignant and non-malignant, are attended by the same symptom. In pyelitis, and in almost all organic changes of the kidney, in Bright's disease, and in diabetes, there is frequency of making water. Whenever the

natural characters of the urine are altered before it reaches the bladder, the secretion produces irritation. The fact is worth dwelling upon for a moment. Diluted or watery urine is often regarded as unirritating; on the contrary, it is not generally well retained by the bladder. The bladder is never so content as when it contains a urine of average, or more than average, specific gravity. Some persons, hysterical patients for example, will pass urine which is quite pale, almost like natural water, and the bladder is always more or less irritated by it. Of course in diabetes you have not only the character of the urine altered, but the quantity much increased.

The next question has reference to pain; and when you get answers as to the nature and seat of pain you will begin to see your way towards a diagnosis. In prostatitis there is usually pain at the end of passing water—less severe, but resembling somewhat that of stone; as the bladder contracts, when empty, on the tender prostate. In cystitis the pain is usually before micturition, because the inflamed bladder is sensitive on being distended, and is anxious to get rid of its contents. The pain is just above the pubes. When cystitis is acute, pain may be felt in the perineum also; but in chronic or subacute cystitis it is supra-pubic, and not at the end but at the beginning of making water, unless the prostate is affected, and then the tender prostate gives a little pain at the end.

In stricture of the urethra there is often pain about the seat of the obstruction, an idea of which you may obtain by a simple experiment. If, when passing urine with a full stream, you suddenly narrow the passage with your finger, so as to diminish the stream one-half or more, you will experience an acute pain.

There may be pain with hypertrophy of the prostate, inasmuch as this is frequently associated with chronic cystitis, when the pain is before making water, and not afterwards—differing in that respect from stone. The bladder wants to get rid of its contents, and can do so but slowly, on account of the enlarged prostate, which stands as a barrier in the way. During its first contractions, which expel but little urine, there is pain above the pubes and deep in the perineum; but when third or a half of the contents has issued, the patient is relieved.

I shall not dwell upon calculous disease of the urethra. The calculous is only a temporary lodger there, and as it can often be felt externally by the hand there is rarely any difficulty about the diagnosis. Calculous disease of the prostate is also rare. I shall not complicate what I wish to be a simple matter by dwelling upon it, but call your attention to the commoner condition of calculus in the bladder. Here the pain is quite distinct in its character; it is felt at the end of passing water, because the

bladder being emptied, the rough surface of the stone is left in contact with the mucous membrane, doubtless that covering the neck of the bladder, which is unquestionably a sensitive spot. As soon as sufficient urine has trickled down into the bladder to separate the coats from the stone relief is obtained. Then the pain at the end of the penis, within an inch of it, about the base of the glans. Furthermore, the pain is increased by movement; in other complaints it is not necessarily so. Put a patient in a rough-going vehicle, or make him jump from a step, or perform any rapid movement, and instantly he feels severe pain, probably at the neck of the bladder, but also at the end of the penis. In prostatitis, inasmuch as the neck of the bladder is involved, there is usually some pain at the end of the penis, which is a reason why chronic inflammation of the prostate produces symptoms much resembling those of stone.

With regard to calculus of the kidney, I have little to say about it here. Of course you have pain referred to the locality, right or left, not often to both kidneys; there is tenderness also, and much increase of pain on movement. It is usually on one side only, and perhaps more frequently on the left than on the right side.

One cannot, perhaps, say much about any characteristic pain in connection with tumours. They may be situated in any part of the bladder; may obstruct the urine more or less, and accordingly as they produce cystitis and obstruct the flow of urine pain will be experienced.

The next question is as to the character of the urine itself. Now, suppose your patient has told you that he has frequency in passing water, pain at the end of the penis and at the neck of the bladder, and that the pain and frequency are aggravated by movement. You may begin to imagine, "Perhaps the man has stone in the bladder, and I shall have to sound him." Two questions only have already put this probability in your way, and you interrogate as to the character of the urine. See how this carries you a step further. We recommence our list as to this inquiry. A preliminary remark, however about examining urine. I do not propose to teach you here a systematic mode of doing this. It is not in my department, and would only be repeating that which it will be your duty to learn elsewhere, and I hope you will do so thoroughly. But there is this hint which I may give with respect to it. Whenever you want a specimen from your patient to examine, do not tell him to send you a bottle of it passed in the usual way, or you will get a mixture of often doubtful value. What you require is the secretion of the kidneys, plus only anything there may be in the bladder; you do not want it complicated with anything which may come from the urethra. Let the man pass two or three tablespoonfuls

through the urethra first, so as to sweep out whatever may be there, which may be put into a separate bottle, and then you will get a specimen—at any rate one of which you will know the source. You will have the renal secretion plus anything in the bladder. Suppose the man has gleet or chronic prostatitis : there will then be a quantity of muco-purulent matter in the urethra. If all this be carried into one vessel with the urine, how will you determine the different products, and decide by the microscope or by the eye what has come from the urethra, what from the prostate, and what from the kidneys ? You cannot do it ; but if you get rid of the source of error by flushing the urethra, so to speak, and emptying the contents into a separate glass—say a wineglass,—then taking the bulk or remainder in a tumbler, you will generally have a sample of urine that you can do something with. If I felt disposed to indulge you with gossip, I could tell you stories of the gravest blunders committed by not attending to that simple point. I can at all events tell you that I have more than once known a learned practitioner treat a patient for pyelitis who had nothing but a profuse discharge from the urethra ; how the urine had been sent twice a week in a scrupulously clean bottle, and because a quantity of pus was found in it, the patient, who had some symptoms corroborating that view, was treated during some months for pyelitis ; how a surgeon at length found out that the whole of the matter came from the urethra, so that when the urethra was flushed into the first glass all the matter was there, and that the remaining urine was clear and healthy ; and, finally, that the "pyelitis" soon disappeared under local treatment of the urethra. I do not know whether anyone else may tell you of that simple mode of determining this matter. I am always calling attention to it ; and I will assume that in the future you will none of you make such a mistake as that I have mentioned.

Referring first to prostatitis, it is always associated more or less with shreds in the urine, which come from the prostatic part ; and if you separate the urine as I have told you, you will find that the whole of the thick matter will be in the first glass, while what remains behind will be clear. How would it be with regard to calculus ? You might have muco-pus in the first glass but you would have more in the second from the bladder. Scarcely ever have you calculus in the bladder without having some muco-pus from the bladder itself. Occasionally, but very rarely, do I find a man with stone in the bladder having perfectly clear urine. Not commonly do I sound a man for stone who has clear urine, unless he has marked symptoms, because the presence of stone in the bladder almost always gives rise to a certain amount of cystitis, and there is deposit in consequence. If the patient passes shreds of thick matter in the

first glass, and the urine left behind is clear, and has symptoms like those of stone, rely upon it he has chronic prostatitis.

The character of the urine in chronic cystitis is well known. There is at the bottom of the vessel a thick mucilaginous deposit, which does not issue in a stream, but falls out in a mass. In acute cystitis, the urine is cloudy, and there is a considerable deposit of pus. In stricture of the urethra, unless chronic cystitis has been set up, there is no deposit from the urine. Here the character of the stream is important. If, when the patient is passing urine, you see a very thin, small, spluttering stream, or urine issuing only in drops, you will know that there is an obstruction, most likely stricture; because, although in hypertrophy of the prostate you may have the stream much diminished, it will be a stream which falls downwards from the organ. In stricture, force may be brought to bear on the stream, so that, however small it may be, it is often fairly propelled; but in hypertrophy, in which the expelling apparatus is involved, the muscles cannot act, and therefore, however large or small the stream, it generally falls perpendicularly. In calculous disease of the bladder there is nothing to note about the nature of the stream. With regard to the débris of tumours found in the urine, the microscope sometimes, not often, throws light upon their nature. No doubt you may see cancer-cells in the urine, but it is most difficult to identify them. I have seen such cells declared to exist by good observers in cases in which cancer was not p esent. Young pavement epithelium is easily mistaken for them. Going upwards from the bladder, we may note pyelitis, more or less chronic—a disease in which the condition of the urine is only one symptom among many others which must be observed before arriving at a conclusion.

The next question is, "Do you pass blood?" and this will bring you very near indeed to an opinion on most cases—not quite, because in any case you may require to sound. In prostatitis there is often a little blood at the end of micturition, as in stone; in cystitis there is not necessarily blood, unless it is acute and far advanced; in stricture of the urethra there is not necessarily blood, and in hypertrophy of the prostate not necessarily. You may have it or not, often only as the result of instruments. It tells most in the question of stone. Just as in phthisis, a large proportion of patients have hæmoptysis at some time or another; so in about the same number of cases, say four out of five, there is some blood with vesical calculus.

I want you to pay particular attention to these questions, because they underlie much of what I shall have to say. What I have to add with regard to observation by the eye, by the hand, and by instruments, will come under each particular subject hereafter, and I will only briefly allude to it to-day.

By the eye you observe mainly whether the bladder is distended or not, and you are assisted by palpation and percussion. You examine the perineum and scrotum also, with a view to extravasation of urine, perineal abscess and fistula, &c. And now we come to the question of instruments. Suppose such a case as that to which I have already referred, in which there is frequency of passing urine, pain at the end of micturition, pain on any considerable movement, thickening of the urine, blood passing occasionally, but more on movement, you regard it as highly probable that the man has stone. You cannot arrive at a certainty without instruments. You may have almost all these conditions in certain changes of the kidney and in renal calculus, and you cannot distinguish them unless you skilfully explore the bladder with a sound. When I claim great value for this instrument, quite understand that I am by no means desirous that in the case of every patient who comes to you and complains of some frequency in making water, or pain in the act, you should say "Lie down and let me pass an instrument." Perhaps the surgeon may be apt to abuse a little his power of passing instruments; it should never be done unless it is absolutely necessary. I hold that an instrument, *per se*, is an evil—a very small one or a considerable one, according to the manner in which it is employed,—and is never to be used unless there is good reason to believe that a greater evil is present which it may mitigate or cure. But when your patient has the symptoms named, you will be doing him an injury unless you resort to it. In cases of stricture the instrument is also necessary; and so in ascertaining the condition of the bladder, whether it is full or empty. A man may make water very frequently, strain hard, and be very certain that he has emptied the bladder, and be quite deceived. How can you tell? There may be a tumour simulating a distended bladder. You cannot know whether the bladder is emptied unless you pass an instrument. Many a man has had an instrument passed into the bladder, and a quart of urine has been found to be left behind, when his own sensations led him to believe that he had expelled every drop. We shall see more of this when we come to the question of retention and hypertrophy of the prostate.

As we are now on the question of diagnosis, I take the opportunity of showing you that the eye may be assisted to a certain extent by what is called the endoscope, which is simply an instrument that we have long been in the habit of passing into all the cavities of the body—the ear, the vagina, the rectum —for the purpose of bringing reflected light to bear upon the interior of those cavities. For some years it has been attempted to do the same with the urethra. It is sixteen years ago since I first saw the endoscope applied to the urethra. This was in

the hands of Mr. Avery, of the Charing-cross Hospital. As I was turning my attention somewhat to this subject, he asked me to see some of his patients, and a new instrument he was then making. He showed me a long tube, precisely similar to this which I hold in my hand, with certain arrangements enabling one to see deep portions of the urethra. He showed me cases of stricture, but I do not think he looked into the bladder. He paid a great deal of attention to the subject, and the instrument was brought by him to a certain state of perfection; unhappily, however, his death occurred shortly afterwards, and the thing was lost sight of here. Various attempts have been made with the same object, long before and since, but I do not know that there is anything on this table very superior to what Mr. Avery showed. Within the last few years M. Desormeaux, of Paris, has paid great attention to the endoscope, and has perfected one of his own, consisting of a similar tube, but with different appliances. The various modes in which light is applied constitute the differences between the various kinds of endoscope. In all of them there is a tube of this description to pass into the cavity. Five years ago I had an endoscope of M. Desormeaux's, and exhibited it in the hospital—the instrument which you see here. Dr. Cruise, of Dublin, has brought it to greater perfection, and has produced a better instrument than we heretofore possessed. I may tell you at once, that if a man has a good and a tolerably practised hand, with a fair share of intelligence, I do not think he will gain a great deal by the endoscope; and if he has not, I think it will be of no use at all. There are some few cases in which he may find it of value; but do not expect that the endoscope is going to work any marvels in the diagnosis of surgical diseases of the urinary organs. In nineteen cases out of twenty you ought to be able to arrive at the necessary information without it. And it is not the easiest thing in the world to apply. As already remarked, a man should not be put unnecessarily to the pain and inconvenience of a sound or a catheter; but examination by the endoscope is a somewhat more irritating and tedious process. In certain exceptional cases, in which you are unable to arrive at a conclusion without it, you may employ it to some advantage. This patient before you had an exceedingly bad stricture of the urethra, which I cut internally last Tuesday week. He is now perfectly well. He could not pass a drop of urine before the operation, but now he is able to pass it naturally; and you will agree with me that a great deal must have been done last Wednesday week to make that change. I cut through the strictures deeply, and now we shall see whether we can find the cicatrices. I shall use Desormeaux's endoscope, illuminated by Dr. Cruise's lamp. You see we have now made a careful and prolonged examination.

The urethra is of a more dusky red about the part which has been affected, but that is all which can be observed. Changes in the colour and texture of the mucous membrane of the urethra and bladder are those which are most easily seen, and which are of the most importance to note. The orifice of a stricture may be sometimes seen, but the result is without practical utility. A stone in the bladder may be easily seen, or rather the small portion of it upon which the end of the sound impinges; but I have never gained anything by the sight. A calculus smaller than a pea may be easily found by delicate sounding—an audible note elicited from it; more easily than you can see it through the endoscopic sound. I may mention that no one has yet been able by its means to identify the veru montanum, and if you cannot see the veru montanum I think it is quite possible that minute pathological changes will often escape you.

Mr. Baker has been good enough to send up another and very simple endoscope, designed by Mr. Warwick, for exhibition. It may be used with ordinary gaslight or with sunlight. It certainly seems, on comparison, to effect nearly as much as the larger and more elaborate instrument.

Stricture of the Urethra.—It is, of course, assumed that in treating a case by means of dilatation, whether continuous or simple, the instrument has fairly passed through the stricture, otherwise the stricture is not dilated. But all cases of stricture are not so easily disposed of. It often happens that at the first, second, or even third trial you do not get the instrument into the obstructed part, or only partially so; or it leaves the canal altogether and goes into a false passage. At all events the instrument does not go through the stricture, and onwards, as it should do, into the bladder. Now that is a condition of difficulty which opens a new subject for us to-day. We have now to deal with more difficult cases, those in which all your care, and experience if you have any, are needed. It was said by Liston, that of all operations in surgery there were none so difficult, none which required so much patience and care, as the passing of a catheter in an obstinate and very narrow stricture. That statement is in his last edition of the "Practical Surgery" (p. 476), and you can scarcely have a higher authority for the fact there mentioned.

Now there is one word which is often used to describe stricture, to which I take a great objection, and I think the sooner it is expunged from the vocabulary of surgery the better. Such a stricture as that of which I now speak is often said to be "impermeable." What is impermeable stricture? Why, first, it does not denote a quality necessarily inherent in the stricture at all, but rather the quality of the surgeon. For, you see, a

stricture may be "impermeable" as regards A, but not "impermeable" as regards B, who may pass the instrument easily enough. But secondly, it is a contradiction in terms. Stricture is a *narrowing* of the canal; it is not an obliteration of it. There must be an opening, and if there be an opening, there must be room for an instrument; it cannot be "impermeable." It is only a question of the size of that instrument, and of skill or patience in the management of it. The stricture always admits urine, more or less in quantity, to pass through it, and I maintain the truth of the axiom first enunciated by Professor Syme, that whenever urine passes outwards though a stricture, an instrument ought with care and perseverance to be got in. I advise you to believe in that doctrine, not that it is true as regard yourselves at the present time, for I will assume that you have not sufficient experience to be able to pass an instrument through a stricture in all cases. If you are able to do so I can only say that you are quite out of place here, and need not come to learn. It is exceedingly difficult to pass an instrument in some cases, but after considerable amount of experience you will find that there are very few in which it cannot be accomplished. In the treatment of stricture, when you have really a difficult case before you, it makes all the difference whether you act under a belief that it is your own fault if you do not succeed, or whether, on the other hand, you hold the dogma that there are a certain number of cases which are "impermeable" to all surgeons. The man who holds the latter belief will be quite certain in some cases not to succeed, whereas, probably, the man who believes that in all cases an instrument may be passed with time and patience, will be very likely to succeed in all, or at all events will succeed better than the other. "Impermeable" stricture is not heard so much of now as it was twenty years ago. It was fully believed in then, and operations were frequently seen in the hospitals for it; but I will undertake to say that they are very rare now. The operation consisted in passing a large instrument down to the stricture, and opening the urethra upon it from the perineum, and then getting through the obstruction, or by it in some way, if the operator could, into the urethra beyond. It was very seldom that the true passage was followed, but somehow, by dint of cutting, a way was made for the instrument to go from the urethra before the stricture, to the urethra behind the stricture, and it was not a very successful proceeding. That was acknowledged in all books at that time as an operation for impermeable stricture, or as "perineal section." I have never had occasion to perform it, and I believe the necessity for it, if ever existing, is excessively rare. I have already given Professor Syme credit for having first enunciated that doctrine, and he has stoutly

maintained it, to the great advantage of patients who suffer from stricture.

But you may have complete obliteration of the urethra, which, as before said, is not stricture. This is very rare; but it occasionally happens, and chiefly after injury in the perineum, as by the breaking of a vessel, or any wound there cutting into or across the urethra. If the opening remains pervious, and gives exit to all the urine, a cicatrix occurs involving the anterior opening of the divided urethra, which is then closed altogether, and so the canal is obliterated in front of the fistula.

Now how are you to deal with a really narrow and difficult stricture? I will assume that you have a case in which you have tried to pass an instrument three or four times, and have failed. The first thing to do is to see the patient make water. Your failure to pass the instrument may not have arisen from narrowing of the urethra; there may have been some false passage made. It may be that there is no stricture at all. No greater failures are made than with those patients who have little or no stricture, either from the surgeon not knowing well how to pass the instrument, or from there being a false passage into which he gets, so that he does not enter the bladder at all. You first of all see the stream of water, and judge by that what size of instrument is to be used. And always let the instrument correspond with the size of the stream which you see. It should correspond in this way, however,—it should be a little smaller. You know, of course, that when a current of water passes into a narrower passage than that in which it has been flowing the stream is more rapid than before, and when the passage becomes larger it flows slowly again; so that the size of the stream as it flows from the orifice is not to be taken as the precise measure of the calibre of the narrowest part of the canal. You should, therefore, take an instrument somewhat smaller than the stream. There is a patient in No. 10 ward, whom some of you have seen, who does not pass a stream at all; the urine is only a succession of drops. How small then, in such a case must be the instrument that is employed. Then there is one very important thing to be considered in connexion with small instruments—namely, that no more dangerous weapon can be introduced than a very small catheter, unless corresponding care, delicacy, and gentleness are exercised; indeed, it is impossible to be too careful in employing it. You see how easy it is with such an instrument as that which I hold in my hand to get into one of the lucunæ or into any false passage, or to drive it through the soft walls of the urethra into the tissues outside: therefore it should always be used with the utmost lightness of hand. You must not hold this slender catheter tightly, resolving to get it

through any obstruction; but it must be held so lightly that if it meets any undue resistance it will slip through the fingers directly—anything rather than wound the urethra. I certainly cannot advise you to try such an instrument until you have had some fair amount of practice with a larger one. In cases of difficulty, a small gum instrument is often not of great service. I have been advocating gum-elastic instruments as the rule; but if you have to deal with a very tight stricture, and fail to pass the flexible catheter after one or two trials, you must use a small silver instrument.

Now, one word upon the subject of using force. *Under no circumstances whatever should force be used in the introduction of an instrument through a stricture or into the bladder.* That is my dictum to-day. Years ago it was matter of discussion how much force should be employed; more years ago still you heard of people using instruments with great violence. Now I am perfectly satisfied, and I believe all modern surgeons will tell you, that no force is to be employed. It is very difficult to say what one means by force; but what you are to understand is, that no kind of weight or pressure is to be put on the instrument which can by any possibility carry it out of the canal; and very little force will do that. The more difficult the stricture the less are you to dream of using force. You will remember that the urethra is possibly of full size up to the point of narrowing, and hence it may be very difficult to find the opening. If you use force. you will perhaps perforate the soft walls on either side, and this done, you have increased your difficulties; because if you make a false passage, the point of the instrument is much more likely to be caught in it than to go through the strictured part.

Remember, also, that a stricture in the course of a canal does not necessarily follow the exact direction of that canal. It may be a little tortuous; may be on this side of the axis or on that, not necessarily in the middle. You may see this sometimes in the dead body; and you may infer it from experience on the living. When you have to deal with a very narrow stricture, take a small instrument which you intend to guide. Do not rely upon mere groping to find the orifice. First of all it is exceedingly dull work to be constantly groping. You should always adopt some method—any method you please, provided that it shall seem to you exhaustive of the different modes of exploring the urethra. Some of you saw the plan I adopted with a patient to-day; and I believe that to be the best plan. You should go systematically to work, and slide your instrument from the orifice of the urethra down the one side or the other. This diagram will show you what I mean. If I go down on this side I shall probably not get into the stricture; but if I slide

down gradually on the other I shall probably get the instrument in, because you see there is less obstruction on that side. Begin, then, by the roof. The roof is the firmest part, and, by following it, will be most likely to carry the point in. The floor on the contrary, is the softest, loosest, and most spongy part; and will be most likely to yield to the instrument, and give way. If your first effort does not succeed, take the right side; if that does not do, take the left; if that does not do, take the floor. I know no other method so calculated to help you through a difficult stricture. If you are very careful, you may make the attempt in this manner for twenty or thirty minutes without doing any damage; but if you find the patient suffering much, if you are losing patience, give it up, or you will probably make a false passage, and increase the difficulty tenfold.

[Speaking of the diagnosis in cases of stricture, the writer observes:]

One rule is to be invariably followed: whenever you examine for stricture, take a fair-sized instrument, say No. 8 or 9. The patient may remonstrate, and will probably say, "That wont pass; it is useless to employ so large an instrument." Tell him that you do not propose to pass it, but only to discover where the obstruction is. For if you use a small instrument at first, it may pass through the stricture, if one exists, without detecting it; but if the large instrument goes on easily into the bladder, you have the satisfaction of telling your patient that he has no stricture, and you look further for the cause of the difficulty. Whatever, then, a man may tell you, and however small the stream may be, take an instrument not less than No. 8 or 9, pass it gently down the canal, and if there is a stricture the instrument is arrested, and you will find the exact position of the obstruction. Notice the point where it exists—four inches, or five, as the case may be, because it will be useful afterwards in dilating to know its exact locality.

Now, in passing the instrument, you may meet with circumstances which may mislead you. I have spoken to you of error on the part of the patient, and I am bound to say that the surgeon who is not much practised in these matters may also be deceived. What are the sources of fallacy to which he is exposed? How is it that he sometimes fancies there is stricture when there is not? There are some kinds of practice in which you may be hereafter placed that do not afford the opportunity of often seeing this disease, and in such it is no great discredit to a man to think that he has found stricture when none exists. Not, of course, if he is a professed surgeon; then it will be a great discredit to him. But if he has very little to do with this

kind of thing, he may encounter some difficulty in the urethra, and he may suppose, but erroneously, that it is due to stricture. Now, I want to guard you all against this; for though you may not all be operating surgeons, I want you not to leave any course of lectures which I may give, without knowing precisely what are the sources of fallacy, so that I may not hear of any of you hereafter making such a mistake as that to which I have referred. Let me remind you again of what was said about the urethra being a closed canal. I am not going to draw you a diagram of the urethra such as you see in anatomical books, in which it is represented in section as an open passage; for it is never in that condition except in the act of micturition. First, close to the meatus is a source of fallacy—I mean the lacuna magna. Then five or six inches further on the bulb joins closely the membranous portion, and the canal, from being wide or dilatable, becomes very narrow. Lastly there is a source of difficulty at the neck of the bladder. Those are the three points at which persons may be mistaken in passing an instrument, and form erroneous notions in consequence respecting the presence of stricture.

Nowwhenever you pass an instrument, do not let your thoughts revert to those anatomical diagrams representing the urethra as an open tube; bear in mind that it is simply a closed passage, so that nothing is easier in passing an instrument, if you are out of the line, than to find some obstruction in the folds or lacunæ of the mucous membrane, since it has more distensibility at some points than at others. Of course if it were simply an open tube there would be less difficulty in going on, but, as it is not so, the point is liable to hitch against the soft parts on one side or the other. First, as I have said, it is quite possible to get stopped at the very outset, which is embarrassing to a beginner, by engaging the point of the instrument in the lacuna magna. Whenever, then, you begin to pass an instrument, let your first thought be to keep on the floor, so as to avoid that obstacle. You wish, of course, to pass it well for the patient. Perhaps he has had instruments passed before, and you desire to succeed at least as well as the preceding operator. Now there is nothing which a patient appreciates so much as the easy passing of an instrument. It is a disagreeable operation, and if you pass it more easily than other persons, you will probably retain your patient as long as he requires assistance of that kind. If you stick and get into the lacuna magna at the outset, he infers you to be a bungler, and, perhaps, will not come to you again.

Now you see represented in this diagram the bulb of the urethra. The canal is more distensible at this point, and when you come to the deep perineal fascia the canal is much less dis-

tensible. Practically, therefore, it is much wider there than it is at the orifice, and when you get the instrument at that point it is apt to hitch. This is the place where almost all the false passages are made; the instrument is driven out of the canal down below the urethra, it being mainly at the floor that the canal is so distensible. The section of the corpus spongiosum is wider below than above; the texture is soft and spongy. The urethra corresponds in distensibility to the soft structure outside, and although the instrument gets smoothly down to this point it may not go on into the membranous portion. Now take care, at first, to get the point of the instrument so turned up as to avoid this lower part. Nothing is so good as a well curved instrument to avoid that obstacle. I am in the habit of doing this in the case of out-patients. I like to take a student who has never passed an instrument before, and I say to him, "Pass this bougie (a straight or slightly curved one) into the canal." He passes it, and invariably when he arrives at the membranous portion, stops. I then take the same instrument, give its point a curved form, then it passes immediately into the bladder. As the instrument goes in it keeps close to the roof, instead of engaging itself in the distensible part of the bulb.

The last obstacle is at the neck of the bladder, and so common is it, that you often hear of "stricture of the neck of the bladder" —a thing which never exists. There never was a stricture even in the prostatic portion. "Stricture at the neck of the bladder" was a household word some years ago, and even now you sometimes hear of it; but there is no such thing. It is simply because there is sometimes difficulty in passing the neck of the bladder that it came to be regarded as a locality of stricture. In this case also a well-curved instrument is the best thing to go in. Let me recapitulate shortly the three sources of difficulty: First, the lacuna magna, which is avoided by keeping on the floor of the canal; then the narrow membranous portion at the bulb, which is avoided by keeping the point of the instrument well up; and the same with regard to the neck of the bladder.

I was going to read to you the notes of some cases of stricture in the ward upstairs, but I will not do so, because you have seen the cases, and know pretty well what they are; and I shall only refer to some of them as we go on. One has been treated by continuous dilatation, and another by internal cutting. Then we have several patients treated by common dilatation, and it is of common dilatation that I have to speak to you to-day. Suppose that you have passed a full-sized instrument, and found that it stops unmistakably, say, at about five inches, the most usual place; you will then, of course, have to pass smaller instruments to see what will go through it. You have diagnosed the fact of the stricture, and the situation; but

you do not know the calibre. You ask the patient to make water. If he cannot do so, take a small gum-elastic instrument, say No. 1 or 2. You pass No. 1, and that perhaps goes through easily; then No. 2, which goes not so easily; then No. 3, which goes in tightly. No. 3, then, is the calibre of the stricture. Now you have all the main facts about the stricture that you want to know. The man's general condition, the frequency with which he makes water, the amount of chronic cystitis—all these are questions to be considered; but it is only with the mechanical part that I have to deal to-day. Lastly, we will assume that he has only one stricture; for a patient may have two, but this is exceptional. Now, what is the treatment? First and foremost, dilatation—dilatation always—dilatation without exception, whenever it will succeed. It is always to be tried first, because it is the simplest and easiest mode. If you find a man with a stricture ever so narrow or tight, by no means think of operating till you have tried whether dilatation will succeed. What is dilatation? A mechanical process of stretching this organised lymph, which forms bands round the canal at the strictured point. Now, suppose you have been able to pass No. 3 bougie or catheter rather tightly through the stricture and into the bladder, to be perfectly sure that all has gone right; then you say to the patient, "That is enough for to-day; come again in two or three days' time for a larger instrument." Now I advise you on such second or third day not to commence with the largest instrument previously passed. Having passed Nos. 1, 2, and 3 on the first occasion, you should now take Nos. 2, 3, and 4; and on the third occasion 3, 4, and 5; and so on: always beginning below the point you had attained on the previous occasion, making the smaller instrument a sort of *avant courier* for the larger one. Further, never let the instrument remain in the urethra; withdraw it at once: leaving it there simply increases irritation, and does not augment in the slightest degree the dilating power.

Now comes the question of the kind of instrument to be employed. The great principle which underlies all mechanical treatment of the urinary organs, whether for stricture or for hypertrophied prostate, for retention of the urine or for stone—the one great principle which must decide for us the question of the kind of instrument to be employed, is this: *all instruments are to be considered evils, more or less, never to be resorted to unless a greater evil be present.* The passage of an instrument of any kind into the healthy urethra must, *per se*, be a source of irritation. Try it yourself; and I advise you to do it, if you wish to pass an instrument well; for I hold that no man should pass an instrument for another until he has passed one for himself. Of course the amount of irritation will depend in great

part on the manner in which it is passed, and on the kind of instrument employed. Let us consider, to use a commercial simile, that in the case of your patient there is a "debit and a credit side" in all treatment. You intend, beyond all doubt, to effect some real good—that is to the "credit side" of the account; but you cannot do it without making some irritation in order to gain your end—that is an entry on the "debit side." Be careful, then, that you constantly bear in mind the latter fact, and make it your business to diminish that "debit" as much as possible. Do not pass an instrument unless there is some good reason, unless there is some evil, for the sake of curing which it is worth while to incur a little irritation. Acting upon this principle, you will choose such an instrument as you know by experience or otherwise to produce the least possible irritation. And this leads me to the question of difference between solid and flexible instruments. Here I feel that I am treading on delicate ground; and I will tell you why. First of all, no one has been a greater advocate than I was some years ago of solid instruments as against soft ones, influenced by the traditions of this place, which are entirely in favour of the former. I can give you the reason for that. The great master-spirit of this place, who has been dead some twenty years or more, the man who gave the tone to the place, and educated almost all the elder men here—I mean Liston,—declared his preference for the solid instrument in very strong terms. It is just twenty-one years ago since I sat in this room and heard him deliver a lecture on that very subject. His powerful advocacy of the silver instrument, and the contempt he had for others, were matters of notoriety. Starting, then, with such views, and regarding him—as every one does to a certain extent the man who teaches him well and fairly what he learns—for a certain time as an oracle, I was strongly in favour of the solid instrument as against the flexible. But what is much more valuable than any oracle, whoever he may be, is a large personal experience; and this has taught me that, beyond all question, the flexible instrument is the best—if only you know how to use it—for the treatment of stricture, and for all maladies of the canal, whenever it is available. I am so certain of this that I have no hesitation in saying that a great part of the success of any man who has much to do with this subject will depend upon the use of flexible instruments as against solid. No patient will ever allow a surgeon to pass for him a solid instrument if you have passed for him a flexible one as easily as you are bound to do. It gives him so much less pain, and produces so much less irritation. To continue my commercial simile,—it puts so much less on the "debit side" of your patient's case; you get so much more of advantage, and so

much less of disadvantage. I confess, then, to a considerable change of opinion even since I published my first work on the subject; and I do this without the slightest shame or the slightest repugnance. I hold that the end of life in this world is not achieved without change in our opinions. You may rely upon it, with to regard to any subject whatever—whether politics, or religion, or our own proper profession,—if we hold the same opinions at forty years of age as we did at twenty, and, perhaps, looking forward, I may say, if we hold the same opinions at sixty as we do at forty, we live to very little purpose. It is an error to look for a life-long "consistency" in matters of opinion from men who think for themselves in whatever department their teaching may be. You must expect them to progress, or they will be bad teachers, just as I hope you are all progressing now. I have said this because I know that so much might be quoted from what I held fifteen years ago in contradiction to what I am now saying. If I did not state this, you might ask me why, having said so much in favour of the silver instrument, do I now say so much in favour of the other. You have my reason: it is simply that I have learned better.

There are two kinds of flexible instrument, the English and the French. Inasmuch as the French instrument possesses more flexibility than the English, I often prefer it. Perhaps it is right that I should qualify a little what I have said. I believe the flexible instruments are much better now than they were in Liston's time; and I think that, if we had had the good fortune to retain him so long (without taking credit for making a shrewd guess), he would have changed his opinion too. This is the kind of flexible instrument much used in his time. It is called a bougie; and properly, since it is simply a kind of wax candle, and is, in my opinion, a very imperfect instrument. You can bend it into any form by warming, but it is a very inferior instrument to what is generally used now. The gum-elastic or English flexible instrument is very valuable on account of one quality which it possesses, and which does not belong to the French instrument—*i.e.*, it will preserve any curve, when cooled, which you choose to apply to it under the influence of heat. If I want a small curve, I take the instrument, put it in warm water, give it the curve required, put it in cold water, and the curve is fixed or set. The French instrument is exceedingly flexible; you

may wind it round your finger without difficulty. And it has another valuable character—namely, its peculiar tapering point. Now a tapering point *per se* is often undesirable in the urethra, because it is very liable to get into some lacuna. It would be an advantage if you could ensure that it did not do this; and this liability is provided against now very ingeniously by means of a little bulb at the end (see illustration). The long tapering extremity guarded by the bulb insinuates itself through a healthy urethra, or through one not greatly contracted, in the most certain and easy manner. Such an instrument as this may be passed by the patient himself without difficulty. Indeed, it may almost be said to be "surgery made too easy." The merest tyro can pass it in nine cases out of ten, although he might not manage the tenth. It is one of the most extraordinary instances of English conservatism that these instruments are found in so few hands. They are, however, at last being made here. For years it has been necessary to send to Paris for them; but a demand is arising now, and they are at length manufactured in this country. If you will try the experiment on yourselves, you will find the *bougie à boule* traverses the urethra almost without your knowing it; and it requires no knowledge of the canal in order to use it. Now it may appear to you very heterodox, but I advise you, in passing an instrument, to forget all about your anatomy. You are taught it over the way, and it is most important that you should know it; but in passing an instrument forget all about the different regions. Think nothing about the deep fascia, the membranous portion, or the compressor urethræ. A solid instrument is especially dangerous in the hands of an anatomist; he will push it the way he thinks right, as if all urethras were exactly of the same form, and did not vary as much as noses do, or other features. This used to be the pretext for preferring the solid instrument: it was said, "You want to know exactly your anatomy, and pass the instrument accordingly." I pity the patient who has a solid instrument thrust into his body by a knowing man at anatomy. You want an instrument that you can use most delicately, holding it lightly between the finger and thumb, withdrawing it or changing its direction as soon as you are able to perceive an obstruction. Your hand is to be educated for the power of delicately perceiving the characters of the passage by means of the instrument within it; and rarely, if ever, are you to push a solid instrument in any given direction preconceived to be the right one. If you wish to get the largest amount of dilatation and the minimum amount of irritation, this is unquestionably the instrument for you to use.

There is one other point to notice with reference to French and English instruments,—I mean the gauges. Our numbers

m are from 1 to 12. Here is No. 12; and you generally consider, ch when you have reached that size, that you have completed the sul dilatation. In England we cannot be said to have a uniform est rule; all our measurements are very arbitrary. One maker achiev one scale, and another another; and the Scotch scale differs with t one and a half from the English; so that the patient who gion, es No. 12 Scotch, takes only a 10½ English. Our more exact at fort hbours over the Channel adopt the millimetre, and the mber represents the exact size, so that when I have named he size, I have named the exact calibre or magnitude of the urethra. Instead of 1 to 12, the French have 1 to 30. They egin lower, and go higher than we do, and the steps are more gradual. In this way irritation may be lessened. You may pass, for instance, a No. 4 English very easily, and a No. 5 with great difficulty, or not at all, and an intermediate one might be the proper size. Their numbers 3 to 21 are about equal to our 1 to 12; showing you at once how much more gradual the range is. No. 1 is one millimetre in circumference; No. 2 two millimetres, and so on; so that the increase in size is uniform as well as gradual. If I have a patient who will take No. 21, I know that his urethra admits an instrument twenty-one millimetres in circumference, and of course seven in diameter. I advise you in this, as in other matters, to be cosmopolitan in your views, and to adopt improvements from all quarters.

I have told you that simple dilatation consists in passing every two or three days a larger instrument, until you reach the highest. In many cases all goes on smoothly from the beginning to the end. Then you teach the patient to pass the instrument for himself, and he does so afterwards once a month, or once in six weeks, to maintain a sufficient calibre.

I will touch but lightly on "continuous dilatation," or the tying in of the instrument. There is a patient up-stairs, who is now undergoing it successfully. You have tried we will suppose, the simple dilatation, and have not made the amount of progress desired, or, perhaps, the patient's avocations may make it necessary to have a more speedy cure. In either case you may say, "If you can give me ten or fourteen days in your room, not necessarily in bed, but on the sofa quietly at home, I can almost certainly bring you from the smallest number up to the highest,"—that is, by "continuous dilatation." In the first case the instrument is simply introduced, and at once withdrawn; in the last case you tie the instrument in, and allow it to remain for several days. You tie in a small catheter, which if possible is to be gum elastic, and so that it just enters the bladder. And you should always take care that it is small enough to pass easily, so that it lies loosely in the canal. These three conditions being granted, this is one of the safest and best modes of

treating stricture. The patient upstairs has finished the process, and to-day the house-surgeon tells me that he has passed No. 11 with ease. The man has not now the slightest pain or frequency of making water, and he has not been so well as he is now for twenty years. He says he is as well as ever he was in his life; he came here in as bad a condition as it is possible to conceive. He had been treated as an out-patient, and, making no progress, I advised him to come in, and try continuous dilatation. I repeat, three conditions are necessary to success: You must have a flexible instrument; the point must not be far in the bladder, and it must not fill the stricture, because, remember, it is not a mechanical process; you do not want to distend the stricture as you would a lady's glove, but you let the foreign body lie in the passage. If you leave a No. 1 in for a sufficient length of time you will be able, when you take it out, to pass No. 10, without using the intermediate numbers. You do not leave it there because the stream of urine would soon wash it out, and it is necessary to put in a larger one to fill the canal a little more. If when you change the instrument you put in one quite as large as the stricture will admit, you produce pain and irritation, and infringe the principle I have laid down, so that the progress is less satisfactory. In continuous dilatation, as well as in simple dilatation you adopt the plan which gives the least amount of pain and irritation to the patient, putting as little as possible to the debit side, and as much as possible to the credit. The operative proceedings to be applied to the treatment of stricture I shall next speak of.

Now suppose, you have a false passage. Of course I will assume that you have not made one,—you will be much too careful for that; you are perhaps not the first surgeon who has seen the patient; some one else has seen him before you, and made a false passage.

You can easily conceive how difficult that false passage must make it to get the instrument in. It is only to be done by being very careful in avoiding the side on which the false passage is. A false passage commences usually on the floor, and no doubt for the reason mentioned, the structures below being looser than those above. When you examine a patient with a false passage, you probably find the instrument passing up to its very end, and no urine flows. Hence the notion that there is stricture at the neck of the bladder. When the instrument is thus passed, put your finger into the rectum, and you will know instantly whether there is a false passage; for if so, there are only the coats of the bowel, which are very thin, between your finger and the instrument, so that you feel it very distinctly. But if the instrument is in the right passage, you feel the whole thickness of the prostate, not always very considerable, between

it and your finger, still quite enough to show that you are in the right path. It is almost always in the bulbous portion that it leaves the urethra and gets under the prostate. What you are to do, then, is to withdraw the catheter two inches or so, then pass it on again, keeping as close along the upper part of the urethra as you can, ascertaining by means of the finger that the instrument is not passing into the old route. It will be very likely to do so, because it is much more easy to get into a false passage than into the right one. I have devoted as much time to this subject as I dare. I have only given you these general hints; you will arrive at the rest yourselves by practice. There is very often a case of false passage in ward No. 10, and I wish you sometimes to verify the position of the catheter when it is in a false passage; to observe how very little tissue there is between the finger and the instrument. Then when it is properly passed, you feel the thickness of the prostate intervening. You can only appreciate this by the touch, and will learn very little more by talking of it.

One word with regard to the injection of oil. When you have a very narrow stricture to deal with, instead of oiling the instrument, it is as well to throw half an ounce or an ounce of olive oil into the urethra, holding the syringe well round the meatus. It is easy to insinuate half an ounce or an ounce of oil through a very narrow stricture. It lubricates the parts, and sometimes the urethra is distended with the oil, so that, if you can cleverly retain it with the finger and thumb, you may introduce the instrument when you have been unable in any other way. This is worth noting. It does not do well when there is much bleeding or the tissues are torn, but when it is not so the plan is sometimes useful.

Now suppose that, following these hints, you have carried the instrument through the stricture; there is a sort of grasp which is quite unmistakable. That is a sensation which you are always content to have, because, feeling the catheter "held" by the stricture, you know you are through it. But that very "grasp," which you are so satisfied to have, makes it less easy to manipulate the point of the catheter after it is through the stricture, and you have sources of danger to encounter in the urethra beyond. Thus, the mucous membrane, being often reticulated, with dilated lacunæ, it is easy to engage the point of the instrument in one of those, and make a false passage. Be careful never to use force in this stage, and, even after you are through, to get gently and quietly into the bladder.

Let us pursue the case one step further. Suppose you have got your catheter in at last after much difficulty. Do not pull it out. You will say: "I had trouble enough to get it in; and

now I will tie it in at once." It is safe to do that, although it is a metal instrument; and you may keep it forty-eight or seventy-two hours before removing it. Do not be in a hurry to do that. You will be excessively disappointed to be obliged to repeat your task; and it will be very disagreeable to the patient. Keep it in about three days; and then you will be able to change it for a gum-elastic instrument. You then go on with "continuous dilatation" as already described, increasing the size of the gum catheter from time to time. You get up, perhaps, to No. 10, and tell the patient, "This is a great point gained;" but in ten days or so, to your great disappointment, the stricture will admit nothing larger than No. 2 or No. 3. Now it is clear that you have to do with what is called "resilient" or "contractile" stricture. It is not a question of calibre so much as a question of contractility. The narrowing has gone back almost to that condition in which it was at first. You can now only get in, say, No. 2. It is useless again to attempt dilatation. This is precisely that kind of exceptional case for which you reserve other means of treatment. Understand that you may have stricture very narrow, and be able to dilate it, and the dilatation shall be permanent. On the other hand, you may have stricture which will admit even No. 5 or No. 6 catheter easily, and yet the man will scarcely make a drop of water. We had an instance of that in the ward the other day. No. 6 instrument was passed; but the man could make no water until I had operated. These contractile strictures have been the plague of surgeons from time immemorial. If you go back to old records of surgery—how far back?—some hundred of years—you will find that these cases have exhausted the wits of surgeons to the present day. All kinds of things have been used in order to overcome the difficulty. I cannot tell you half of the matters which have been put into the human urethra. I suppose the human stomach has been made to receive more abominable things than any other receptacle in or out of the human body. But if you consult the old surgical authors, or even some modern books, you will see that the urethra has been used nearly as roughly: and that is saying a good deal. Verdigris, savin, the salts of all sorts of metals, everything that could irritate, or that could be imagined to be disagreeable, has been employed to cure these unfortunate strictures. At the present moment I need not tell you that some surgeons have employed nitrate of silver and caustic potash—not at all mild remedies either of them. Now the whole question of chemical irritants, as applied to stricture, I shall dismiss with the following words: I believe them to be unnecessary, undesirable, and dangerous. Most modern surgeons, both in this country and abroad, have pronounced against the use of caustics and chemical

irritants in the treatment of stricture. I shall not pursue that question further.

Then what have we left? Several mechanical modes: we may rupture, or overdistend, or cut these unyielding and contractile fibres, which constitute the stricture. Urethrotomy, as it is called—division of the strictured urethra by some form of knife,—is, perhaps, all things considered, the most universally employed in such cases. Now there are two kinds of urethrotomy—external urethrotomy and internal urethrotomy: the external applied from the perineum; the internal, by means of the knife or some other instrument applied within the urethra.

I speak first of internal urethrotomy. There are two modes of doing it. You may cut the stricture from before backwards, or from behind forwards, the latter being the method most commonly adopted.

First, suppose you have a stricture at or near the orifice—a very undilatable part. You get in an instrument like that which I hold in my hand—a small bistourie cachée; project the blade and draw it outwards, dividing the obstruction. Nothing can be more simple. By means of a screw, you can raise the blade to a small or to a large extent. It should be used so as to make a rather free incision, which is perfectly safe. As a rule, the nearer the stricture is situated to the orifice of the urethra, the more necessary is it to cut, and the safer is it to do so. The further the stricture is from the external meatus, as a rule, the less necessary it is to cut, and there is rather more risk in doing so. All strictures at the meatus, and within, say, about three inches of it, which have resisted dilatation—and these generally do so—should be divided. You can dilate them, but the effect is temporary; and you can divide them with ease and perfect safety.

Secondly, with regard to strictures at a distance of five inches, the reason why it is somewhat less safe to cut them is on account of the large amount of erectile tissue at that point. In my own experience of sixteen or seventeen years of these operations, I have never lost a case by either internal or external urethrotomy, either in the hospital or out of it, and I have had a great many cases; I cannot say how many,—perhaps a hundred. Sometimes there may be a dangerous amount of fever, and, rarely, cystitis; much depends, I need not say, on the way in which the thing is done. External urethrotomy is usually known as Syme's operation. It is necessary always, in that case, to pass a small grooved staff or director through the stricture, to cut down upon the perineum, as in median lithotomy, and divide the stricture completely by cutting freely into the groove of the staff. That is very seldom done now. It was

much done twelve or or fifteen years ago ; but other means have superseded it. Occasionally the operation is still performed, chiefly when there are perineal fistulæ which require to be laid open. After doing it, a catheter is tied in for forty-eight hours, and an instrument to maintain a good calibre must be passed from time to time afterwards. Now for internal urethrotomy in these cases of stricture in the bulbous urethra, this instrument which I hold in my hand is a very good one. I used it for a case here about a month ago, and the result was simply perfect. It has a small bulb at the end, concealing a small sharp blade within, which you pass through the stricture. You can then, by means of the bulb, feel exactly the position of the stricture. Having done this, you unsheath the blade about half an inch beyond the stricture, to the extent required, and draw it forward, dividing the contracted part freely. In that case, also, you should tie in a catheter forty-eight hours, and after that pass an instrument every third day, then every week, and so on. Here are instruments for cutting from before backwards, and of various kinds ; but I prefer the others as a rule.

We next come to the mode of rupture ; and here I will show you an instrument which bears the name of Mr. Holt, of the Westminster Hospital. He has brought the instrument into notice, and the mode of using it is his own. It was invented by M. Perrève of Paris, more than twenty years ago. He used it mainly, but not altogether, for simple dilatation : Mr. Holt uses it otherwise. He passes it through the stricture, and then, instead of passing at different times a succession of tubes of gradually increasing calibre, he takes the largest tube at once, and forces it down the urethra along the central guide, so as at one stroke to split everything that happens to obstruct the passage of the instrument. Mr. Holt does not put in an instrument afterwards. When the operation was first introduced, I was strongly repelled by the violence of the proceeding ; but I examined some of Mr. Holt's cases with him at the Westminster Hospital—now some ten years ago—and was surprised to find how few bad results followed. Hence I tried the plan, and use it occasionally for a urethra which has some two or three contracted points—a condition rarely met with,—so as to deal with them all with certainty. Rupture, however, is too rough a mode, in my opinion, for most cases ; and I prefer to carry a fine blade carefully through the fibres of the stricture, and believe it to be the best, and the most enduring in results. But the popular sentiment about a knife cannot be ignored. The British public is not partial to a sharp edge, and is glad of almost any substitute—a feeling one can quite understand. So that it is not always possible to do the best thing, and we

have to select that which is next best. You will, then, probably find the proceeding by rupture a useful one when dilatation has failed. That operation suggested to me some years ago a different method—namely, one which I have called "over-distending" the stricture. It is simply this. Here are two blades, as in the instrument used by Mr. Holt, but these two blades can be separated for a considerable interval at one point, and as slowly as you please. In practice I do it very slowly, so as to rupture as little as possible, and to over-distend as much as possible, the structures which form the obstruction. There is this fact also to be noted: I wish you to remember that the bulbous part of the urethra—the usual position of stricture—is also the most distensible part in the natural condition of the canal. Supposing the external meatus to be about No. 12 (English scale) in calibre, the bulbous urethra admits at least No. 20 or 24. Hence it follows that no kind of dilatation or operation which is limited in extent by the size of the external meatus more than half restores the urethra which has a stricture at the bulb. It is on this account that I distend the contracted part to at least this, or even to a larger size, by means of the instrument in question. I have now used it many times, and it is certainly attended with good results. It is to be used only for strictures within the bulb. I have heard of its being employed for those within three inches of the meatus: it is a mistake to use it for such, which, as I said before, ought to be cut. This operation requires more care than the other. Mr Holt's operation needs no care after the instrument is once in place; it is extremely easy to use it. If you once get the instrument in correctly, a single impulse of the hand forces the tube through; and it is certainly tempting on that account. And for a considerable time afterwards you have good results from these operations. I think, however, that internal urethrotomy gives results which are more enduring than any other; but it is infinitely more difficult to perform properly, and, without doubt, it requires a practised hand.

Briefly let me, at the close of this subject, remind you that in all cases of impeded micturition, attention to the general health often aids in a considerable degree to mitigate the local troubles. Do not overlook the state of the digestion. If this is unsatisfactory, if the bowels are unduly constipated, the troubles of the bladder and urethra will be much increased; and frequently it happens that a mild mercurial, followed by a dose of Glauber's salts, or of Friedrichshall water in the morning, gently unloads the liver and bowels and greatly relieves the most distressing symptoms.

There is only one other word I have to say: do not be partisans of any single method. You hear one surgeon say: "I

always follow such and such a method ; there is nothing like it." Or another, that he always adopts the proceeding of M. Civiale, and a third that of M. Maisonneuve, and so on. There has been great fertility in inventions of this description, especially in Paris, and you may see from several of them very excellent results. Do not confine your selection to any one method, whether I or any one else recommend it ; but have every resource at your disposal. If you have much to do with stricture, or with such complaints, I can only tell you you will want all the resources within your reach. Consider them carefully, and select for each individual case that method which appears in your judgment to be the best adapted for it.

[It was stated on the great authority of Sir Benjamin Brodie, that "when the hair becomes grey and scanty.....the prostate gland usually, I might perhaps say invariably, becomes increased in size ;" and that is the impression which a large portion of the profession have respecting it. Sir Henry Thompson examined two hundred cases of death above fifty-five years of age, and finds that the above maxim is very far from true.]

I found that about one in three exhibited after death some enlargement of the prostate. But of those do not suppose that anything like a large proportion manifested symptoms ; for only about one in seven, not more, had symptoms of the complaint. So that you see it is not to more than one in (let us say) ten men who live beyond fifty-five years of age to whom you expect to be called to afford any relief for this affection. That, no doubt, is a large number. If you suppose that one man in every ten approaching sixty years of age has symptoms of enlarged prostate, you will see at once how often, if you have anything like a large practice, you may be called to attend to these cases.

We will now consider one or two anatomical points connected with enlarged prostate. This organ is, as you know, composed of two lobes and a median portion. Now, the part affected with hypertrophy very much influences the results in relation to the function of micturition. It is not necessary that there should be much enlargement of the prostate in order to produce very severe symptoms. On the other hand you may have a very large prostate, and may have almost no symptoms. Almost the largest I ever saw, as big as a small cocoa-nut, produced very little obstruction to the flow of the urine. Thus, if the median portion of the prostate is only slightly enlarged, there may be retention. Let this figure [diagram] represent the two lobes, and this the median portion. If there is a small nipple-like projection at the median portion, just filling the internal orifice of the ure-

thra, that may be quite sufficient to prevent every drop of urine passing by the natural efforts. You will remember, then, that if you have occasion to examine a patient, and you find a very large prostate, it does not necessarily follow that he should have great difficulty in passing his water; and, on the other hand, although you may find inappreciable enlargement by rectal examination or otherwise, you may not therefore conclude that all his troubles, and they may be considerable, are not due to this complaint.

I speak next of symptoms. An elderly man comes to you and says that recently his water has not passed so easily, that it has issued in a dribbling stream, and he cannot propel it: that he requires to micturate a little more frequently, especially in the morning—probably two or three times while he is dressing; after which it becomes less troublesome, but at night it is rather more so than during the day. Then, if he does not say much about pain—which, of course, will excite your suspicion of calculus or some other complaint,—you will say, "This is probably a case of enlarged prostate." You do not necessarily proceed at once to pass the catheter, but you will ask the four questions already referred to. You will ask how frequently he makes water, and then, in connexion with frequency, whether the water ever passes without his being aware of it, or without his willing it. In many advanced cases you will find that some urine passes during a violent effort, such as coughing, or at night during sleep. Next you ask for pain, and if any, whether before, during, or after the passing of the water. If before, and the patient is relieved by making water, it is probably hypertrophied prostate. If it is after, you may expect calculus, which comes into contact with the mucous membrane of the bladder when the water is gone; whereas if he has a distended bladder, as he is likely to have with enlarged prostate, it is painful as distension occurs, and becomes less so as the urine passes off. Then you enquire about the character of the secretion, whether it is clear or cloudy. In most cases, at the commencement it is clear. In a great number of cases of prostatic enlargement, although the bladder has not been emptied for a month or even a year, the water is clear. On the other hand, if it is an advanced case the water will certainly be cloudy. And while you are talking of that you will also ask about the stream. You will generally find that it is a dribbling stream, different from the stream in cases of stricture. In stricture the stream is often propelled exceedingly well, although it is no larger than a thread, and so long as there is a stream he can act upon it by will, so as to make it stronger; whereas, strain as he may, in prostatic enlargement he often cannot influence the stream except for the worse. Then you ask the fourth question, "Do you pass

blood?" Usually, in the early stages, the reply will be in the negative, although a little may appear after much exercise, so far suggesting stone.

Then you complete your diagnosis by mechanical means, and for this purpose you will first use a catheter. You should invariably make the patient pass water before you begin, because your object is not merely to ascertain whether enlarged prostate exists, but, what is much more important, how far it is a barrier to the exit of urine from the bladder. The great point to him and to you is, not the size, or condition, or shape of the prostate, but to what extent is it a barrier to the exit of urine. It is the quantity of urine left behind which will determine future treatment. I should advise you to use a gum catheter well curved, and certainly not a small one. As in stricture, always begin with a catheter of not less than 8 or 9, and of course without a stylet. In passing it keep the shaft well back in the groin, so as to maintain the curve. As soon as you have arrived at the bladder, carefully empty it, and note the quantity withdrawn. It may vary greatly, from an ounce up to almost anything you please. I have drawn off six pints, but that is a very large amount. You may find commonly from six to twenty ounces.

Now with regard to the employment of the instrument for patients with the symptoms described, do not forget that frequency of passing water, and still more the passing of it involuntarily, indicate the necessity for the use of the catheter. It is remarkable how common are the errors, not merely of patients but of practitioners on this point. They are apt to be misled by the fact that the patient insists, "I do not make too little water; I am making water too frequently, and too much of it, and I am sure my bladder must be empty. Tell me how to retain my water, and I shall be much obliged to you; don't think of drawing it off." It is surprising how that sometimes influences the practitioner. Nevertheless these are the very circumstances in which you should pass the catheter. Always bear this in mind, (and I wish, figuratively speaking, to render that sentence in the largest capitals,) that Involuntary *micturition indicates Retention, and not Incontinence.* There are a few exceptions to the rule, but very few. Most of the mistakes that are made on this point arise from the use, or as I shall show you, the abuse of the word "incontinence," which means of course, that the bladder is empty; and certainly when the bladder cannot hold its contents its condition is rightly described by the word incontinence. Now that happens only in very uncommon, but well-defined circumstances, such as in some cases of cerebral or cerebro-spinal paralysis, and in rare injuries to the neck of the bladder, and in these the urine runs off as fast

as it comes from the ureters, the bladder having ceased to act as a reservoir. You see this one external physical sign is the same in these cases and in those in which the bladder is distended with urine; that, is, there is urine dribbling off by the urethra. But mark how totally different are the two conditions in question. Whenever, then, you meet with this involuntary flow of urine, miscalled "incontinence," do not confound it with the condition in which the bladder is empty. Rely upon it the bladder is full, and the only way of relieving the patient is by the use of the catheter. I lay great stress upon this, because I have seen lives sacrificed to a forgetfulness of this point. I have made post-mortem examinations of persons who have died from the effects of retention undiscovered during life, misunderstood because the urine constantly passed off, as it was supposed, "so freely."

Now we know that our views of things, and our consequent acts, are very much determined by the manner in which we use and apply words respecting them, and it is impossible to be too clear and defined in all language, especially in that which relates to pathological conditions and surgical practice. I cannot express to you how strong my sense is of the importance of this matter; hence I have made it my constant business to point out the common misuse of terms in connexion with this subject.

First, then the term incontinence, which means the bladder is empty, or "can't contain," should never be employed by you to denote the phenomenon that the patient's urine flows involuntarily; for, as we have seen, in that condition the bladder is generally full. It is better to use another term—viz., "overflow"—to denote that state; and then, remembering always my maxim, that "overflow indicates mostly retention, and not incontinence," you will never make the fatal blunder I have spoken of, and which I assert to be so common. This, too, assimilates our usage very nearly to that of French surgeons. The French, with their more logical use of language, speak of the bladder as "engorged" and "overflowing," but never as incontinent, except to denote that rare condition in which the bladder is always perfectly empty. I have, therefore, long been in the habit of denoting a bladder which is full, but allows surplus urine to run off little by little against the will of the patient, as an "engorged" bladder, and the phenomenon thus described as "overflow;" and I hope you will do so too. This brings us to another misapplication of terms. In this country the condition of the organ just alluded to is often called "paralysis" of the bladder, and the unfortunate word leads to mistakes in practice. The bladder is rarely paralysed. I know nothing of it except as an effect of spinal or cerebral changes. The bladder is never by itself the subject of paralysis, meaning,

of course, an affection of the nerves either central or peripheral. It may be unable to expel its contents, because there is mechanical obstruction, as enlarged prostate, stricture, or impacted stone, or because the muscles have lost their power of contracting from long over-distension; but this latter condition is "atony." This inability is in neither case due to impaired nervous supply—a subject to be considered at some subsequent meeting.

After this digression, which its importance must excuse, we will go on to complete the diagnosis. While the patient lies on his back you place your finger in the rectum, and examine the size of the prostate, whether it is very tender, and whether the enlargement is more on the right or the left side. Of course you do this as gently as you can. The finger should be well oiled. The best position for the patient is lying on the back, because you can press the other hand above the pubes, and gentle pressure there brings the bladder and prostate near the finger, and you can ascertain whether the bladder is distended or not.

We shall now come to the treatment. The *medical* treatment of enlarged prostate may be dismissed in a few words. There is nothing to be done for it—that is, you cannot diminish the hypertrophy. There is often temporary enlargement from congestion; and that you can do something for. But hypertrophy cannot be diminished by any known means. All sorts of things have been tried: iodine in all its forms. And there is scarcely anything in the Pharmacopœia that presented a chance of doing good that has not been tried. We must simply say that, for the present, we know no means of combating the enlargement itself. But much may be done by way of palliative to the results of the complaint; and much of this treatment is mechanical. This will consist, first, in relieving the distension by the catheter. I have reserved for this place what I have to say about the instrument itself. The reason for preferring, as a rule, the gum catheter for this purpose is as follows:—Different curves are required for different patients; and the English gum catheter I will back against any other instrument, English or foreign, for general use. Unlike the French catheter, which is admirable for softness and elasticity, you have the power of making it assume any form you like—a quality, perhaps, not so often rendered available as it might be, but which to my mind is of immense service. The instrument-maker generally curves the catheter in this way [diagram]: the point straight, and not well curved—the worst form in which you can put a catheter for use. For prostatic enlargement you require a catheter well curved *to its very point*. You should keep the instrument on an over-curved stylet for a month or so

before employing it; and you will then find it easily assume the proper form, when you will pass it, as I need not say, without a stylet. If you require a stiff instrument, a silver catheter should be selected; not a gum catheter with a stylet in it. To return: you want the point of course to be carried over the obstruction formed by the enlarged prostate; and as the heat of the urethra always relaxes the curve, whatever it may be, by the time it arrives at the neck of the bladder, the ordinary gum catheter, as you have it from the maker, becomes nearly straight, and will not pass over the enlargement. [Diagram.] Now, when you have a catheter which has been well over-curved for a month or two, you remove the stylet and turn back the shaft, so as to undo the extreme curve and produce an ordinary one. And what happens when you pass it? In spite of the heat of the urethra, the catheter has a tendency to curve more, instead of less, as it passes down the passage. And this is precisely the difference between success and non-success. That little manœuvre I regard as of extreme value. It is a very simple thing: keep the catheter over-curved—not for stricture, but for enlarged prostate;—then turn back the shaft immediately before using. The curve gradually increases as it goes onwards, and it passes over the enlargement into the bladder. This is so simple that it seems scarcely worth making so much of; but I can only tell you that I know nothing of its kind that exceeds it in value.

Then there is another thing: you may want a special curve for a particular case. We have silver catheters with various curves. Here are several which are very useful. But the English gum catheter possesses a quality which, as I have before told you, is not found in any other: put it into warm water and bend it into any form you like; then dip it immediately into cold water, and it will maintain the required form. But the best form so produced may easily be spoiled by your mode of using it. Of course the curve must not be altered while the instrument is passing through the anterior part of the canal, for it is at the posterior part that this form is required: the shaft of the catheter must be kept closely back in the groin, and the penis brought round the curve, so as to preserve the latter until it reaches the deep urethra, when by well depressing the shaft the point will rise over any obstruction into the bladder

The general treatment is not to be disregarded; and here I shall defer a good deal of what I might otherwise say until I come to speak of chronic cystitis, which will occupy a subsequent lecture. Cystitis is associated with so many diseases of the urinary organs that I may as well refer to the treatment under that head, instead of taking it separately, and recapitulating it in connexion with each disease. But as far as the general treatment of prostatic enlargement goes, the main thing is to

prevent local congestion. You must tell the patient above all things to avoid anything like chills affecting the pelvic region, sitting on cold seats, exposure to cold; too much excitement, sexual or otherwise; long journeys, riding in jolting carriages,—all of which tend to produce pelvic congestion, and to interfere materially with the condition of the prostate.

One other point has relation to the action of the bowels. You may make a man with enlarged prostate very comfortable if you keep the bowels in gentle action. If he has constipation, and scybala lodge in the rectum, their presence is often the source of great discomfort. Sometimes a simple enema of warm water gives instant relief; but, if necessary, the action of the bowels must be provided for by means of gentle laxatives, such as senna, manna, bitartrate of potash, sulphur, or by sulphate of soda; anything which will act mildly, quickly, and without irritation, will keep him in a very different condition from that which is associated with habitual constipation.

I will devote the few minutes that remain to those cases of prostatic enlargement in which the difficulty of passing the catheter is considerable, and there is retention of urine. You may have a patient in whom prostatic enlargement has manifested itself rather suddenly; he may have had symptoms which he has not noticed, but he is suddenly attacked with congestion, he cannot make water, and he is in great distress in consequence. It is not a question how long a time is to elapse before the catheter is to be passed; you should relieve him at once. You find there is a distended bladder, evidenced by dulness of percussion above the pubes. Possibly, before you, others may have come, and you may be called to pass the catheter in circumstances of some danger to life. Now, in the first place, you should be careful with regard to position. I advise you to pass the instrument with the patient in the lying position first, if the bladder is very large; if it is not very large, perhaps it is as well, or better, to pass it standing. You can empty the bladder better in the standing position; but if you find that the bladder is very large, make the patient lie down before you draw off the water. I have known great mischief arise from drawing off a large quantity of water from a patient when in a standing position. I have even known death occur suddenly from this cause. If I had time, I could tell you of a case in which a charge of manslaughter was brought against a surgeon in a court of justice in relation to such an occurrence. The circumstances were all well known to me, for I was there to defend a brother practitioner. In that case a catheter had been passed in the upright position, and the patient fell dead from syncope, when six pints of urine had passed; just as a patient with ascites might do if you tapped the abdomen in that same position. No doubt it was

an error, but nothing could be more monstrous than to make it a matter of action at law. It is a very instructive case, and I mention it to show that in cases where the bladder is large, especially in old men, you may have fatal syncope arising in the way I have described. Take care also, in these circumstances, to draw off only part of the urine; and after a quart or so has passed, wait a little before you empty the bladder. If you fail to pass a gum-elastic instrument, by all means use a silver one. The silver prostatic catheter is sometimes essential; that is, one which is much longer and has a larger curve than the ordinary instrument. Sometimes, when these fail, a catheter with a short beak, like the sound or lithotrite, will pass easily. And always remember that anything like obstruction at the end can only denote that you are out of your path. No force should be employed. It is not a narrow passage; it may be a little close, perhaps, when you reach the prostate; but if you find any obstruction, you should withdraw and find another route, to the right or to the left. Never use force under any circumstances whatever.

If you have had much trouble in introducing a catheter you must sometimes leave it in. As a rule it is not good practice, but if an instrument must remain there, that made of vulcanised india-rubber is by far the best; it has generally to be mounted on a stylet for the purpose, which is withdrawn afterwards. It is the least irritating kind, and scarcely ever becomes coated with phosphates.

Subsequently, as the canal recovers, should the bladder not regain its tone, and the catheter is therefore necessary to be used two or three times in the twenty-four hours, you will probably in the course of time teach the patient to relieve himself; and he will often, with a little tuition, succeed remarkably well. But it may happen that the catheter cannot be passed, and the subject of operation for the relief of the patient must be considered.

Retention comes before us in three typical forms, each requiring a different species of treatment. There may be some instances which cannot be absolutely so classed, and some the characters of which partake in part of two classes; still, for convenience, it is well to adopt this classification. First of all, you may have retention occurring in a young and healthy man who has no stricture; next, it may occur in an older man who is the subject of confirmed organic stricture; and, lastly, it may occur in a man who is neither young nor hale, and who has no stricture, but has an enlarged prostate. Of the last I have nothing to say; we have already discussed that subject, and the mode of relieving retention in connexion with enlarged prostate. But I shall ask your attention to the two other conditions—namely, retention

from inflammatory swelling, and retention arising from organic stricture.

With regard to the first kind, you will probably learn a history something like the following. Within a month or six weeks perhaps, the patient, who is generally a young man, has had gonorrhœa. He has obtained considerable relief from treatment, and has in consequence allowed himself some relaxation of the regimen to which he has been lately submitting. Thus, perhaps, he has indulged a little in the use of alcoholic stimulants, has taken some unusual amount of exercise, a game of cricket or the like, and, after being overheated, has been sitting on a cold stone or damp grass; or, lastly, he may have indulged in some emotional excitement. Under those circumstances what is called "inflammatory stricture" may be produced. Now let me say, repeating myself slightly, that that condition has no right to the name of stricture. First of all, the inflammation is probably at the neck of the bladder or in the prostate. It is difficult to ascertain that, except by inference, confirmed however by a rectal examination; for, happily, we very rarely make post-mortems under such circumstances, as the patient does not succumb to the complaint. But what almost to a certainty takes place is a degree of inflammation and swelling of the prostate, not in the least resembling stricture—that is, it is not a circumscribed narrowing at a particular part of the passage, but a tumefaction of the prostate gland, which prevents the expulsive apparatus of the bladder acting and discharging its contents. That is usually the real condition in what is called inflammatory or spasmodic retention.

This condition of the prostate resembles that which affects the tonsils, and which we call inflammatory sore-throat. Both complaints consist in the enlargement of glands which more or less surround narrow passages, and so interfere with the functions of those passages; both occur rapidly, and may be produced by external cold.

Now, what are the early signs of inflammatory retention? First, there is usually some cessation of the gonorrhœal discharge. Just as in the case of orchitis, where the urethral inflammation is supposed to subside and to attack one of the testes, the inflammation of the prostate is similarly associated with diminished discharge, and if you examine by the rectum a very tender and swollen condition of the prostate will be discovered. Then the stream of urine grows smaller and smaller, and in a very short time the patient loses the power to relieve himself altogether. He is feverish, very restless, and suffers severe pain about the lower part of the abdomen and in the perineum. Those who are the subjects of stricture may have

become in some measure accustomed to difficulty, but when an active young fellow is thus attacked for the first time, he is in a state of extreme distress.

Now as to the treatment of such a case. The patient desires ardently to be relieved immediately, and declares he cannot endure his sufferings. You see him bent nearly double in order to relieve the pressure of the abdominal muscles on the bladder, and he is even breathing shortly and quickly to avoid their action there. The old treatment in such cases, the classical treatment twenty years ago, was bleeding from the arm or perineum, repeated hot baths, large doses of opium, so as to enable the patient to bear the pain and yet to dispense with the catheter. The reason assigned was that in an inflamed state of the canal you might do more harm than good with a catheter, and that it was therefore better to mitigate pain by the means described. I have told you that I dissent from that treatment altogether, although it is still employed to some extent. For, first, you must look at the after-consequences; and if you allow a young man to remain thirty-six or forty-eight hours with an unrelieved bladder because you fear to use an instrument, permanent mischief may be done. I have seen patients who for years have been unable to empty the bladder after treatment of this kind. Extreme and continued distension of that organ sometimes destroys or permanently diminishes its contractile power, and produces a condition which is properly termed "atony of the bladder." Therefore if you pass the catheter, even at the risk of doing a little mischief to the urethra, I am disposed to think you would be wise in incurring that little risk rather than expose the patient to the other danger. But then it ought to be done without such risk. For my own part, I always take a moderate-sized gum catheter—one not larger than a No. 6, as a large one gives in these circumstances unnecessary pain—which has been tolerably well curved in the way I have before described, since it has to enter over a swollen prostate. In this manner there is generally no difficulty in relieving the patient, who is exceedingly grateful for what you have done; whereas if you put him through that long process and he relieves himself ultimately, he thanks you for little, comparatively speaking, and he runs the risk to which I have referred. But in the event of the gum catheter not passing, you should try a silver catheter of the same size. I believe one of the first persons to denounce the old plan of bleeding and hot bathing was Mr. Guthrie. If you turn to the racy writings of that experienced and practical surgeon, you will find an anecdote in connexion with this subject. He relates the account of a visit which he paid to a patient in the circumstances of retention I have described, and the reason, in strong and graphic language, why he then gave

up for ever the bath and bleeding practice, and passed the catheter at once for such cases in future.

So much for the inflammatory condition of the prostate producing obstruction to micturition. I need not refer at any length to spasmodic retention, which rarely happens. At the same time it may be as well to say that no doubt where an inflamed condition of the urethra exists, spasm of the muscles may coexist; but the precise share which each takes in contributing to the result will not influence the treatment.

Now I come to the second form of retention—viz., that depending upon organic stricture. Here we generally have to do with an older man, because it is rare to find a young one suffering from confirmed organic stricture. As a rule, such a patient mostly has stricture ten or twelve years before he gets complete retention. First of all you have to ascertain that it is stricture. You will probably find that he suffers less acutely than in the previous instance, but still a great deal; the progress of the case has been more gradual, and the derangement has not necessarily been brought on by any great or sudden imprudence. He has been passing water with difficulty for weeks or months, and at length some slight thing has produced a condition of absolute retention; the last ounce has broken the camel's back. Or it may not be absolute retention as before; there may be some dribbling, indeed the patient may have been relieving himself in that inefficient way for days, but the bladder is greatly distended, and to all intents and purposes it is a case of urgent retention. You find probably also that the patient is accustomed to instruments. Now what you have to do is to take an instrument of middle size, and pass it down to the seat of obstruction, to see where it is. You will probably find it four or five inches from the external meatus. You should then take the finest gum catheter and endeavour to insinuate it into the bladder, and if you are sufficiently fortunate to do that, you should tie it in at once, so that you may have no further trouble with it. But that is not a very difficult case of retention. Supposing you do not get the gum catheter in, I should then recommend a small silver one, either No. 1, or even smaller, and use it in the manner I have described to you in the lecture before last. Notwithstanding all your care and skill, and those, perhaps, of your friends whom you may have called in, the instrument is still not passed. There may have been false passages (they are easy to make), and there may be such difficulty that it is almost impossible for anyone to pass a catheter after your failure. We then come to the question, What is to be done next? Well then, first, much may be effected for these cases by opium and hot baths. Suppose the water is dribbling off, and you shrink from the *dernier*

ressort—that is, puncturing the bladder, or other operation to relieve the retention of the urine,—a safe middle course may still exist for some of these cases. The patient up to this time may have been exposed to cold; let him have the benefit of a warm bed and hot baths, with large doses of opium,—and you must be very liberal with opium if you use it at all,—so as materially to mitigate the involuntary straining, which he can no more help than he can help breathing, and which is utterly unsuccessful as regards the contents of the bladder, and often makes matters rather worse than better. The result may be that the water will dribble off more freely than before, and you may find after two or three days that it will come in a larger stream, and that then you can pass the instrument without much difficulty. The patient may often be saved an operation thus if there are grounds for declining to perform it. On the whole, however, I do not advise waiting very long; still it is better that the patient should be temporised with in that way than that he should be damaged by an unsafe hand either with knife or catheter. Most men, indeed, are quite sufficiently confident in their powers to rely on instrumental methods when they find that a patient cannot make water. Still if you are convinced that you are not doing any good with the catheter, still more that you are doing mischief, you can in most of these cases have recourse to opium or to an inhalation of chloroform, with hot baths and fomentations, with success as regards the immediate and urgent condition.

But we will assume that you have done all that you can do in this way, and that the question of relief by some other means must be met. The bladder is increasing in size notwithstanding your treatment. You examine the supra-pubic region carefully, and find a tense and perhaps large tumour there, reaching to the umbilicus or nearly so, more like a uterus than a bladder. In some old cases of stricture there is not necessarily large supra-pubic dulness, for the bladder is thickened and contracted. Introducing the finger into the rectum you find there also a swelling produced by the distended bladder, and you then seek to obtain the sensation of fluctuation. If, placing my hand above the pubes, I feel a distinct wave communicated to my other index finger in the bowel, I know that to be a point at which the trocar can be inserted with safety. Also if I find a well-marked rounded tumour over the pubes which is dull on percussion, the bowel around it clear and distinct, I have reason to believe that an operation over the pubes would be successful. Again, the question arises, why not attempt to relieve the bladder by an operation on the urethra itself from the perineum, so as, if possible, to cure the stricture, and at the same time relieve the bladder? Might it not be wise in this manner, as

it were, to kill two birds with one stone, and not be content with merely puncturing the bladder by the rectum or above the pubes.

At this point let me revert to the different practice and different experience of surgeons in relation to this matter. Let me give you the experience of Mr. Liston. He once said, from this chair, that the during the whole of his connexion with the Royal Infirmary of Edinburgh, and subsequently with this hospital up to the time at which he spake—namely, three or four years before his death,—he had never punctured a bladder for retention of urine. On the other hand, there are men living in this town who have punctured a bladder fifty times or more. Mr. Liston meant to imply that a good surgeon ought rarely to find it necessary to resort to any other means than the catheter in circumstances of retention. But do not suppose for a moment that the gentleman I spoke of who has punctured a bladder fifty times does so because he fails to pass the catheter under those circumstances, but because he thinks it wiser to puncture the bladder than to persevere too much with the catheter. Both Lister and Guthrie occasionally performed the perineal operation just spoken of. From the perineum the urethra *may* be reached behind the stricture. Now, without entering into a long discussion on the subject, I may say that this mode has lost favour of late years. It is no easy thing to find the urethra behind the stricture; and a man may make an awkward wound in the perineum, and never hit the urethra at all. Then it does not follow that it should be desirable to divide the obstruction at all, so far as its cure is concerned; for the stricture, when the time comes to treat it, may be amenable to dilatation. The reason why puncturing through the rectum has been done so often by Mr. Cock, of Guy's Hospital, is because he conceived it to be an excellent kind of treatment. He says: "Let us withdraw the urine from the urethra altogether for a few days, and the urethra will recover itself, so that we may be able to cure the stricture with ease." And that is often true. He punctures the bladder by the rectum under the circumstances I have mentioned; and this is his instrument for doing it. In this way, the water not passing by the urethra at all, the urethra is lying fallow, so to speak; and in a short time the instrument can be passed—say No. 2. or 3, or 4 catheter, although before you could not pass No. 1. This, then, is a species of treatment of stricture which Mr. Cock has introduced; and, at all events, he has proved how easily and safely this operation may be performed; he has, in fact, familiarised us with a proceeding which before was often thought a very grave and serious affair.

If, then, you have failed to pass the catheter and the symptoms are urgent, you have two proceedings before you: punc-

turing by the rectum, or above the pubes. By carrying into the bowel your finger—a reasonably long finger—you arrive at a point just behind the prostate. The other hand is placed above the pubes, that the wave of fluid, by its pressure, may be distinctly felt by the finger in the rectum. You are then quite certain of what you are going to do. Along this finger, kept firmly in place, your trocar is slipped, and then boldly but carefully pushed into the bladder. This is always an anxious moment, because, if you have not hit the bladder, it is a serious matter to have thrust this long instrument into the centre of the body and find no urine escaping. The best position of the patient is sitting on the edge of a bed, resting his back on pillows behind, the legs apart, each on a chair; and an assistant by him placing one hand on either side above the pubes, so as to steady the bladder, and press it down towards the rectum. It is well to remember, subsequently, that if the canula slips out, you will not be able to get it into the same opening again. The muscular fibre of the bladder instantly closes, and you have to make another puncture: not a matter of much consequence, but better avoided.

For the supra-pubic operation you divide the structures in the middle line until you reach the linea alba. Then carefully making your way deeper, you will soon discover fluctuation; and having the bladder steadied as before, you will thrust the trocar slightly downwards. In this case you retain the silver canula for two or three days; but you may soon substitute a gum instrument. Now supposing there is a probability that your patient may require this artificial relief by tube for some time, you will, of course, prefer the situation above the pubes, because it is much more easy to wear the tube there than it is in the rectum, where it interferes with the functions of the bowel, and is otherwise much in the way. I have known patients who have passed all the urine through a tube above the pubes from ten to fifteen years, and who lead active and comfortable lives in consequence, the natural passage being completely obstructed. One of them, who had suffered greatly before, and was now in perfect comfort, told me that "he did not know whether this mode of passing water was not preferable to the original one!" That, however, I conceive is quite a matter of taste.

[Speaking of extravasation of urine, Sir Henry Thompson observes:]

Suppose that from bad treatment, neglect, or otherwise, the patient has had no relief, and when you are called in find there is no question of puncturing, for the urethra has punctured itself, so to speak; that is, nature (as in the case of all maladies)

has done something ; often she acts in a clumsy way, but sometimes not more clumsily than the surgeon. There is no question that many who are the subjects of stricture or retention, and receive no surgical aid at all, will lose their lives in consequence ; but they are sometimes saved by extravasation of urine taking place. What then happens is, that behind the stricture a portion of the urethra gives way, perhaps during some violent act of straining, and through the rent so made a quantity of urine is driven with great force into the cellular interspaces. Well, we know where that must go, from the anatomical disposition of the fascia—viz., into the scrotum, up into the groin above Poupart's ligament, and towards the belly. Occurring as it usually does in the bulbous part of the urethra, where the walls of the canal are weak, the urine cannot pass backwards behind the scrotum, or the back part of the perineum ; neither can it get into the thighs, because it is checked by Poupart's ligament. I have seen it rise as high as the chest, and I have made incisions to evacuate it, in a bad case of extravasation, as high as this point. Once taken place, it follows that at every action of the bladder more fluid is driven in with force, so that the cellular interspaces are separated, and the fluid usually finds its way upwards. Generally you ought to know at once by the appearance of the patient what has taken place, although it is possible sometimes to confound it with ordinary inflammatory œdema of the scrotum, for it may commence very gradually and insidiously. In ordinary cases you see a hard perineum, a large red, tense scrotum, the penis swollen, and a red blush perhaps rising over the pubes. In order to ascertain the true state of thing, you must ask for the antecedents, and you will probably learn that there was great difficulty in passing urine, followed by rather sudden relief. When a man has had retention for some days, and extravasation takes place, instant relief is experienced—the frightful want to make water disappears as soon as the fluid finds its way into the scrotum ; but he soon feels new pains, not necessarily very severe, but, what is worse, constitutional symptoms rapidly set in. The poisonous fluid quickly destroys the cellular tissue, so that sloughing soon begins. After forty-eight hours or so, gangrenous discolorations appear, and the urine may find its way into the corpus cavernosum, when a dark spot appears on the glans, showing that the structure of the penis itself is infiltrated. Without detailing the condition which you must have seen for yourselves, and which may be seen now to some extent in a patient in the ward upstairs, let me say, Do not in such a case be afraid of the knife. You have no occasion for a catheter; the urine has found its way into the cellular tissue, and you must let it out as freely as possible. On each side of the perineum make a good deep inci-

sion. You need not limit the incision to two or even three inches; because you are really cutting into urine, not into flesh. The structure is so enormously distended that there is but little flesh to divide; and although the incision may appear very deep and long, when the water has run out it will be comparatively small. The incisions generally bleed rather freely. One may soon lose a pint of blood from three or four incisions. The urine runs out also, and as the distension goes off, the vessels are enabled to contract better; but if you see any little vessel spouting, tie it at once. An incision should be made on each side of the penis, because if it is made in the middle line there is not sufficent communication for the incisions on the one side to relieve the other. Do not be extravagant in these incisions; still it is better to err on the side of freedom than to be too niggardly in the use of the knife. The next day, if the case has done well, you will find the scrotum much less in size, and the parts altogether much less swollen and inflamed. You have now a direct communication through the cellular tissue from the bladder, and, with that rent behind the stricture, as a rule, you will be safer in letting the catheter alone, and permitting the water to drain off. What happens? Why just what happens after puncturing the bladder. When the water flows off by another passage the urethra begins to improve, and in three or four days you will probably have no difficulty in passing No. 3 or 4 catheter. With these patients, bad as they are, prostrated as they are when you see them, if the case has not gone too far and too much gangrene has not taken place, very striking and rapid recoveries often follow. The whole scrotum may slough away, and the testicles may be seen uncovered in the wound, and yet all may heal up soundly and well.

[After the exit of the urine by these artificial channels, some of them fail to heal and remain patent, and thus form urinary fistulæ. In all cases of simple urinary fistula,]

Whatever part of the canal it is connected with, it almost always heals if the stricture with which it associated is dilated. Dilate the stricture, and in nine cases out of ten the fistula will heal. Patients, especially in private practice, are often extremely anxious about the result of an opening in the perineum or elsewhere through which the urine passes; and it is right that you should assure them for the most part, if the stricture is thoroughly dilated, the unnatural passage will heal of itself. But there is another point to be considered in connexion with this—viz., the quantity of urine passed by the fistula, compared with the quantity passed by the natural passage. Of course the gravity of the thing depends very much on the relative proportions passed by the two ways. Usually about three-fourths of the

water will pass by the right way, and one-fourth or one-fifth by the wrong passage. If a large quantity—say three-fourths—passes by the unnatural opening, then probably a considerable amount of stricture is present. Nevertheless, as the stricture is dilated, you will see that the proportion passing through the fistula will gradually decrease until it ceases altogether, and the part soundly heals; but this latter result is achieved only on the condition that you maintain the stricture in a dilated condition.

Now we come to fistulæ which are associated with much inflammation and induration in the perineum. These may be multiple; in which case you may find five or six openings. I have known a man with a dozen, so that instead of the urine being discharged by one stream it flows as from a watering-pot. Even this condition, however, very much improves as the stricture is dilated, and may get quite well; but this is not always the case. Then there are some instances looking much less formidable, like those upstairs, with perhaps only two or three openings, through which the greater part of the water has passed for a long while. You recollect that we fully dilated the stricture in each case; but still no improvement as regards the flow of urine through the fistulæ took place. A No. 12 catheter was passed; but the patients did not get well. The condition of the perineum improved very much; but still more than one-half of the urine obstinately held its erratic course through the perineal openings. Now, what is commonly done in such circumstances? Usually operative proceedings of some kind are resorted to; or, if these have been postponed or rejected, a rather tedious process has been employed. The principle laid down—and I have myself applied it successfully—is that it is necessary to take care that the external openings should be very free, ensuring this either by means of the knife, or by potassa fusa, or by some other means; so that the urine may not be detained in its way from the urethra to the external surface, causing fresh induration or thickening. Next, you may go on to excite adhesive inflammation in the track of the fistula by a hot wire, or by touching it with cantharides or a strong solution of nitrate of silver. No doubt this treatment sometimes succeeds; but it is at best a tedious process. Then it was sometimes attempted to cure such fistulæ by tying in a gum catheter for weeks, or even for months; but this always fails, and for this reason, that urine always finds its way from the bladder by the side of the catheter, along the urethra, and so into the fistulæ, by the force of capillary attraction, and thus the object supposed to be attainable, in reality never was and never could be so accomplished. The practical surgeon soon discovers that tying in an instrument never ensures the transit of

all the urine through it: some will always pass by the side and defeat your purpose. I have, therefore, adopted the plan of teaching the patient to pass the catheter himself; and that is by far the most rapid and the most certain method. With regard to the two cases upstairs, ten or fifteen years ago I should have applied potassa fusa or the galvanic cautery, or something of that kind; but the fistulæ have soundly healed through insuring, by means of the catheter, not, as by the other process, that the urine should percolate rapidly through the perineum, but that it should not pass through at all—in fact, turning the current the other way. You first teach the man to pass a No. 7 or 8 gum catheter for himself—an easy matter enough. He then agrees to pass it every time he requires micturition, night and day. On no occasion is he to permit the urine to flow spontaneously—say during five or six weeks,—not even when he goes to stool; and this is avoided by always using the catheter immediately before. That plan has been followed in each one of the three cases in question, without difficulty, and with perfect success; for each man has a sound perineum, and has now relinquished the use of the instrument.

Now I come to the third form of fistula, that in which there is loss of substance. This class must be dismissed rather briefly, because its full consideration would involve tedious detail of many different surgical procedures. Where you have this loss of substance, a plastic operation of some kind is generally required to fill up the gap which exists. When the opening is small, you may contract it very materially by the heated wire or galvanic cautery, or by any mode which tends to produce a contraction of the tissues. You know that cicatrices which result from burns contract considerably, and you avail yourself of that action in this instance. Most commonly, however, if the soft parts have been largely destroyed, some plastic operation is required for the cure. Thus, on passing a silver catheter, when a portion of the urethra has sloughed away, you may see perhaps a quarter or a third or half an inch of the catheter exposed in the wound. The successful treatment of such cases demands much care and nice management. They do not often come under our notice, and less often do they get completely cured. I have had in the hospital but three or four such cases, in which, by means of plastic operations, the patients have been entirely cured. Some of you saw one last winter: a man who had just between the angle of the penis and the scrotum an opening, showing at least a third of an inch of the catheter, the whole of the floor of the urethra having sloughed away. The operation in that case was one of the most successful I ever saw. The first operation completed it, with the exception of an opening not larger than a pin-hole. What was done

was to pare the edges all round, to get a large flap of skin from the scrotum below, which was brought up to cover in completely the wound, the margins being carefully attached by a number of little sutures. That fistula healed perfectly. And why did it heal? Here is the important point: there was one condition necessary, without which it would have failed. A week or two before this operation, I made the patient learn to pass the catheter habitually, so as to draw off every drop of urine; and finding him thoroughly expert at it, I performed the operation; and for the next month he never allowed a single drop of water to pass otherwise than by the catheter. Had I tied the catheter in, it would not have been sufficient, because the water always finds its way by the side sooner or later. Luckily, he performed his part of the compact to the letter for the stated term, so there was no reason why the wound should not heal there as well as anywhere else. The little tiny opening which remained was perfectly closed with the heated wire, and the urethra can now perform all its functions perfectly well.—*Lancet, Jan.* 11, *Feb.* 1, *Jan.* 18, *Feb.* 8, *March* 7 *and* 21, *and April* 4, 1868, *pp.* 35, 154, 73, 183, 307, 367, 429.

55.—ON THE TREATMENT OF STRICTURE OF THE URETHRA BY THE STRICTURE DILATOR.

By Dr. Robert M'Donnell, F.R.S., Surgeon to Steevens' Hospital, Dublin.

[The question of the advantage of rupture of a chronic stricture by Holt's dilator is one certainly on "the debateable ground." Undoubtedly perfect recovery does not always follow this mode of treatment. The experience of Dr. M'Donnell is very favourable.]

In all the cases recorded in this communication the directions for the operation given by Mr. Holt were followed. In the instances in which the stricture was very close I dilated it with bougies for some days, until a No. 4 instrument could be passed. I always operated upon the patient standing, his back firmly fixed against a wall. The instrument is more readily introduced in this position than when the patient is lying down. I may add that I have never found it necessary to use a dilator with the directing-rod hollow; if the patient has for a time retained his urine when the dilator is fairly introduced into the bladder, I have invariably found the water to flow along between the blades of the instrument, not leaving any doubt of its being in the bladder. In fact, I should lay it down as a rule, never to be departed from, that unless the water flows in this way, so as to make it a matter of certainty that the bladder

has been fairly reached, the dilatation should not be proceeded with.

Although in these pages I mention only those cases which I was able to follow carefully, owing to their being kept within prison walls for some years, yet I may say that besides these I have operated on a considerable number of others, both in hospital and private practice, yet I have never met with any untoward result; in two cases only did any considerable constitutional disturbance supervene, and this subsided after three or four days. When I compare this with the ill effects I have myself witnessed, as well in instances of internal as external section of strictures, and even following too hasty dilatation with bougies, it is, I must confess, perfectly astonishing. In short, were it not for the facts staring us in the face, one could not believe that the forcible splitting of an old stricture could do so much good and so little harm. It proves how essentially tentative everything in surgery is.

When operating on my first cases I had the benefit of Mr. Rawdon Macnamara's assistance. To this gentleman we are indebted for the introduction of this operation into Ireland, and his earnest advocacy has not only won for it a high place in public favour, but has in a great degree overcome the timidity of those practitioners who, like myself, at first regarded with fear a procedure which seemed so heroic.

[Dr. M'Donnell then gives a table of twelve cases operated upon by him at the Mounjoy Prison, and therefore under observation for long periods afterwards. One had a relapse four months after the operation, and one died of cholera; all the other cases were permanently cured.]

In all the foregoing cases I had recourse to an expedient which, I conceive, leaves no doubt that stricture actually did exist. I used a double-length catgut bougie, made with a gum elastic catheter to slide over it—an instrument devised, I believe, by the late Dr. Hutton, and much used by that eminent surgeon.

The catgut bougie (of small size) is first introduced into the bladder; being double the usual length, a long piece projects from the urethra; on this a catheter, open at each end, is slid; the end of the catgut being held, the catheter is slipt on, and is thus conducted along the urethra; if it comes to a dead stop at any point, it there has met with a stricture too close for it to pass. The narrowing permits the catgut to get through it; to the catheter it says no. According to the size of the catheter we know the size of the stricture. No lacuna, fold of membrane, false passage, or enlargement of the prostate can now deceive us. The conducting bougie would certainly steer the the catheter past any such obstructions. I think this method

may be considered a crucial test for the existence of stricture, and it was applied in each of the cases mentioned in the above table. I am then, I believe, justified in stating that stricture existed in every one of these patients, as in no one of them could I at first slip a No. 2 catheter along the conducting bougie into the bladder.

At the time (August, 1863) when I operated upon cases No. 1 and 2, Mr. Rawdon Macnamara and Dr. Cruise were present. They will remember, I do not doubt, that one of these patients made a considerable outcry, and that in his case the thrusting in of the dilator required an amount of force which to me, as a beginner, was alarming. The other patient (G. W.) merely said, "Oh, oh," and looked astonished at the unaccustomed sight of a large stream gushing from a large-sized silver catheter. Very little force was necessary, and the introduction of the dilator was accompanied by a slight snap, as if a thread tied round the urethra had given way. This patient was surprised when he was directed to go to bed, as he said the operation had cost him no more pain than the introduction of an ordinary instrument. This case (No. 2, G. W.) is a most important one. Two months before this he had been in hospital, under treatment by gradual dilatation; six weeks after leaving hospital, with a urethra able to admit a No. 9 bougie, I could not pass No. 2 along the railroad catgut. Yet this stricture, which, under ordinary treatment, has such a tendency to return, is split, with little or no pain, and four years after he is reported as having but slight stricture.

Case No. 5 (P. H.)—Was examined by Dr. Cruise with the endoscope some months after he had been operated upon; indeed this one of the cases alluded to by Dr. Cruise in his classical paper on the Endoscope in No. 78 of this journal. We perceived a transverse slit, somewhat crescentic in form, at the spot where the constriction had been.

Nos. 7 and 8 relate to the same individual. This is the only case in which anything of rapid return of the stricture took place, and as the patient died of cholera within a month after the second operation, and as an opportunity was afforded of examining the urethra, the case is one of importance.

I exhibited the bladder and urethra at the Pathological Society on February 16th, 1867.

The following is the report as published in the proceedings of the Society:—

"Dr. R. M'Donnell brought before the Society the bladder and urethra of a patient who had suffered from a stricture, which, shortly before the individual's death, had been treated by the method called 'bursting.' The following was the history of the case:—J. M'C., aged 36 years, was admitted to the

hospital of the Mountjoy Convict Prison, on July 23, 1866. He stated that he had been suffering from stricture of the urethra for six years. He could only pass water in a very small stream, or in drops. At first only a small catgut bougie could be introduced; after dilatation up to No. 4 was accomplished Holt's dilator was introduced, and the stricture was burst. Immediately after a No. 10 silver catheter was introduced into the bladder. All went on well, and four days later the urine was drawn off through a No. 10 silver instrument. He was discharged from hospital on August 3rd. On November 6th he was again admitted to hospital, stating that the stream had become as small as ever. On examination it appeared that it was not possible to introduce into the bladder an instrument larger than a No. 1 catheter. At his own request the operation of bursting was again performed as before. He was dismissed from hospital on December 7th. He died suddenly of cholera on December 22nd. Dr. M'Donnell removed the bladder and urethra, and they were examined carefully by Dr. Cruise, Mr. William Stokes, and himself, soon after removal. The appearance, however, at that time was not materially different from what the members had now an opportunity of seeing. A No. 9 catheter could readily be passed along the urethra. Except for the hypertrophied condition of the muscular coat of the bladder, and the dilatated state of the portion of the urethra behind where the stricture had been, there was no other sign of the disease having existed. There certainly did not appear to be any granular condition of the mucous membrane in the vicinity of the triangular ligament where the stricture had existed, neither was there any marked induration of the submucous tissue. Dr. M'Donnell had at first thought that there was an appearance indicating that the dense fibrous, submucous tissue had been split by the process of dilatation, while the mucous membrane itself had been only slightly, if at all, lacerated. On inspecting, however, other urethræ he found a similar appearance. The interest of this case was, perhaps, in the fact that it showed so little sign of disease remaining. The case was one in which the stricture had unquestionably returned in a comparatively short time after being burst."

Besides the cases given in the table, I met with two cases among the convicts in the Mountjoy Prison, in which the immediate plan failed to afford even temporary benefit. As they were cases of quite an exceptional character, I do not think it would be fair to group them with ordinary cases of stricture.

A military prisoner, committed for an offence of the most serious character, gave the following account of his case:—Some years before he had a bad stricture, which became complicated

by perineal abscess. A surgeon with difficulty succeeded in getting a gum elastic catheter into his bladder, and having succeeded he left it there for a month. When it came to be withdrawn it had to be dragged out with great force, and, as the patient described it, it was encrusted with calculous matter, so that the end of it was as thick as his little finger. Of course the urethra was lacerated from one end to the other, and when he came under my notice the urethra beneath the penis, as far back as the scrotum, was gone. I could, with difficulty, introduce a small silver probe into the orifice, from which the water came. Indeed I could not do so until the patient, holding up the penis, forced out some urine from the little orifice in the angle between the penis and scrotum.

The whole urethra had, in truth, been destroyed, and there only remained a fistulous trajet from the bladder, where the urethra formerly had been. I gradually dilated this channel with bougies, but it narrowed up again with extreme rapidity. At last, in despair, I introduced the dilator, and burst it. It did no harm, but it did no good; the tendency to narrow was as great as ever. The patient was obliged to keep his disease at bay by the daily use of a bougie kept in for half an hour.

The second case to which I allude was a very similar one, in which almost the entire urethra had been made into a fistulous channel by a strong injection of nitrate of silver used to cure gonorrhœa.

No portion of the urethra had ever entirely sloughed away, but abscess had formed, and fistula and the whole length of the urethra was of almost cartilaginous firmness. In this case there was great difficulty in dilating the urethra, and it narrowed again at once. I now also tried the dilator. Here also it did no harm, but it did not all at check the tendency to close again. After one day had elapsed it was impossible to introduce so large an instrument as on the day before.

I am clearly of opinion that it is not, in general, good practice in the treatment of stricture to introduce very large instruments, as No. 12 or 13. I have satisfied myself by observations made by injecting the bladder and urethra with wax, that even in subjects of large size, the portion of the urethra begirt by the triangular ligament rarely exceeds the dimensions of a No. 10 catheter. To dilate, therefore, beyond No. 10 or 11 is, then, to dilate beyond the normal calibre of the canal; the urethra is kept in a state of irritation by this, and generally resents such treatment. I am convinced by experience that when the regular introduction of an instrument is necessary in order to keep a stricture at bay, it is, as a rule, better not to go beyond what may be considered the normal size of the canal at the triangular

ligament, and, as I have said, this does not generally exceed No. 10.

In three cases only of those which I have tabulated was there any decided tendency in the disease to return observed, and in only one to return promptly. To what was this tendency to return so quickly due? This is a question which I am at a loss to answer. I must content myself by recording the fact. Dr. Cruise, whose frequent use of the endoscope has made him familiar with the pathological changes in the urethra, is inclined to believe that the immediate plan will be found best suited for cases of true organic stricture, and that the obstruction arising from chronic inflammatory constriction, produced by the granular state of the canal, is less likely to be benefited by it. Yet we could find no evidence by the careful examination of the urethra in this case, that granulations had existed; but then it must be recollected that the stricture had been treated a second time, and, for so far, with success, at the time when the opportunity of examining it had occurred.—*Dublin Quarterly Journal, Feb.* 1868, *p.* 53.

56.—ON THE USE OF CAUSTICS IN STRICTURE OF THE URETHRA.

By F. Le Gros Clark, Esq., London.

[The following is elicited by the condemnation of the use of caustics contained in a preceding paper (Art. 54) by Sir Henry Thompson.]

I have been in the habit of advocating and practising in public the treatment thus unsparingly denounced, and the results of my experience for many years as a hospital surgeon are entirely at variance with the observation and expressed conviction on this subject of Sir H. Thompson.

I have, of course, often performed perineal section in suitable cases: but I have never divided a stricture within the urethra, because I have never found it necessary or desirable to do so. It has occurred to me that some danger might attend this operation; but I do not venture to assert that such is the case, as I have not tried it. But I have for many years been in the habit of employing potassa fusa in the treatment of old and impenetrable strictures, with the greatest advantage, and without in a single instance causing dangerous consequences. A suitable instrument, and care in using it, together with a little patience, are requisite; but these are elements of success in every operation. Many of my dressers can bear witness to the safe and successful application of this caustic, especially in bleeding

strictures; and I have from time to time recorded such cases in my published clinical lectures.

My apology for this comment on Sir H. Thompson's lecture is, the desire I feel to rescue a valuable remedy in the treatment of stricture of the urethra from being expunged, without protest, from the chapter on diseases of the urinary organs.—*Lancet*, *Feb.* 22, 1868, *p.* 271.

57.—VULCANISED INDIA-RUBBER CATHETERS.

In the case of old men affected with retention of urine without stricture, MM. Richet, Maisonneuve, &c., employ, by preference, vulcanised India-rubber sounds, as do many of our own surgeons. With this flexible and supple sound, says Dr. Victor Reveillout, catheterism, so to speak, performs itself, even in cases where there exists considerable enlargement of the prostate. It seems to find its own way in the urethra with the utmost facility. It causes less irritation than any other when left in the urethra.—*British Medical Journal*, *Nov.* 23, 1867, *p.* 474.

58.—ON STONE IN THE BLADDER.

By Sir Henry Thompson.

[It is stated in books that stone in the bladder is most common in children. This is not the case, it is most common in individuals from fifty to seventy.]

How do you sound? You should employ an instrument like this, with a small, short, curved beak, because it can be turned in any direction. If you take an instrument with a large curve, like a catheter, you are unable to rotate it in the bladder, and hence it does not explore sufficiently.

When I entered this room I asked for the hospital sounds, for I knew I should find among them a good example of what a sound ought *not* to be. Here is one, for example, which no one could rotate, or ever find a small stone behind an enlarged prostate with, except by sheer accident. You will say, naturally enough, "Why are such sounds here, and who has used them?" They were used formerly, and found a good many stones, too, in the hands of our illustrious predecessors. But I will answer for it, they have missed a good many stones also; and this is precisely what I want you not to do. I have no hesitation whatever in saying that more stones are missed in sounding than are found, by the ordinary methods adopted in this country; and that must be the case if a sound of the form of the common catheter is relied on for the purpose. But with an instrument

which has this small beak at the end of it, you can search in every direction. If there is a large stone, of course you can find it with anything; but our great object is to find the small stones. Anybody can find a big stone; the art consists in finding a small one. It is more important to find a small stone, because it will grow large, and may be very formidable to deal with; whereas, when it is small, it is a far less formidable matter. You may promise the patient, in the case of a small stone, that it may be removed without risking his life; whereas, in the case of a large stone, there is always some risk, often considerable danger.

But you have something else to do besides merely discovering the presence of stone. It is necessary to have other particulars respecting it, because the nature of the operation to be performed will depend on them. First of all, it is essential to know what the size of the stone is before you decide on what you will do with it. From the note elicited by merely striking it you can get some indication of its size. It is often sufficient for practical purposes to use a sound (which I have long used myself, and have recently introduced) provided with a little slide on the shaft, which by proper manipulation enables you to ascertain very nearly the size of the stone, as you have frequently seen in the wards.

Then there is another way. You may introduce a lithotrite (which gives, however, a little more disturbance to the patient) and seize the stone in two or three directions, so as to ascertain its diameters.

At the same time you ascertain its nature. A phosphatic stone gives a very different sound from the others. The specimen before me is dry, and therefore will not give the sound to which I refer. When wet, it is spongy and soft, with a rough surface, and always gives a dull note when struck; whereas the uric-acid stone gives a hard ring. Then you will judge partly by the condition of the urine. If the urine is acid, and if, also, uric acid is thrown down, you may conclude that the patient has a uric-acid stone. If so, it is likely he has passed small calculi before. If the urine is very alkaline and there is a great deal of phosphatic matter, you may conclude that it is a phosphatic stone, or, at all events, it is covered with phosphates.

The number of stones is the next thing. Usually there is only one stone, but occasionally there are more. There is a patient here, whom I shall lithotrise to-morrow, who has two rather large uric-acid stones in the bladder. The way to determine that point is this: Having seized one in the lithotrite, you move it gently in every direction as a sound for others. If

then you encounter one on one side and one on the other, you know that there must be at least three stones.

I have spoken of uric-acid and of phosphatic calculi. But it may happen that you have an oxalate-of-lime stone—a very important thing to ascertain. You examine the urine, and see if there is much oxalate of lime thrown down. The patient may have passed a small mass of oxalate of lime before, and you may infer that an oxalate-of-lime calculus exists now; but then it may be covered up with phosphates, and so deceive you.

Having got all these data, the next important question is, what are you to do? Are you to cut, or to crush? You know there are only two modes of removing the stone. You must either make an opening sufficiently large to admit of its withdrawal, or you must crush the stone into very small fragments, so that they may be expelled by the natural passage. It was less important to make a diagnosis of all these points when we had but one operation—namely, that of cutting. Formerly, whether the stone was large or small, the patient was always cut. There was no other way of removing it. Now that we have two operations, it is very important that we should choose the right one, because, let me tell you, if you do not determine pretty accurately the characters of the stone, and select the right operation, you may do more harm than if you cut every patient. If you crush the very large stone, and cut for the very small one, you will have greater mortality than if you simply resorted to the one operation of cutting in all cases. When lithotrity was first introduced, it was rather a clumsy operation; and when the cases were not judiciously selected, when surgeons crushed without making a diagnosis of all these points—crushed stones that ought really to have been cut, and left for cutting stones which might have been crushed,—the entire mortality resulting from operations for stone was greater than previously, when every case was cut. I cannot give you a stronger argument for the necessity of apportioning the operations judiciously.

Now, without taking up your time too much, I will lay down what you will understand to be the axioms which should direct you, in a general way, in making your selection. First of all, I will say that all stones, under puberty, with very few exceptions, are to be cut. Under fourteen or fifteen years of age, stones occurring in the male are to be cut, unless they are very small, and can be crushed, say, in one operation; because lithotrity is not a very easy or successful operation in children, the urethra being small and the bladder very irritable: whereas, as is well known, lithotomy is a very successful operation in these cases. We do not want a better operation, comparatively speaking, and one may be content to let well alone. Not more that one death in fifteen or sixteen cases occurs from lithotomy

in children. I do not think, therefore, we can do better than to cut in these cases, as a rule.

That leaves us all the cases above puberty. Then I will say, in general terms, that all the cases above puberty are to be crushed, with certain rare exceptions. The first exception is in a case of an oxalate-of-lime calculus, which is, let us say, an inch in diameter. Under an inch in diameter you may crush an oxalate-of-lime calculus. I have crushed four or five in my time. The cases are very rare. Two of them were in this hospital. An oxalate-of-lime stone, from the size of a bean up to an inch in diameter, can usually be crushed; above that size no instrument can deal with it, and the fragments will be so hard that the operation might be of doubtful value, even if we succeeded in crushing. That, then, is the first exception to the general law that all cases in adults are to be crushed.

Secondly, a large stone of uric acid, or a phosphatic stone of *very large size*, had generally better be cut than crushed. Mechanically speaking, it is possible to crush any stone whether uric or phosphatic; but considering the number of sittings required, and the amount of irritation produced, the single operation of cutting will be the better of the two when the stone is, shall I say, full two inches in diameter. A stone which is two inches in diameter, either phosphatic or uric acid, had perhaps better be cut. No doubt a rather larger phosphatic stone may be crushed. Here is a very large one, two ounces and a half in weight. The phosphatic stone is very friable, and you may deal with rather larger phosphatic stones than uric acid stones by lithotrity. So much for the exceptions to cutting, regarding the characters of the *stone* itself.

Now what are the conditions on the part of the *organs* which will make it necessary for you to cut instead of crush? If you have a bad stricture of the urethra you cannot crush; if you have a very highly diseased condition of the bladder probably you cannot crush; but the exceptions are very few indeed. First of all I will tell you what are not exceptions, but which are stated to be so in books, and are generally considered as exceptions; because lithotrity has advanced since the time of most books. I have recently crushed a uric-acid stone in a case of organic stricture with small instruments made for the purpose; but the stricture was not a very narrow one. Hypertrophy of the prostate was said to be an exception; it was said that you could not crush under those circumstances. I make no difference whatever in respect of that matter; I would as soon crush in the case of hypertrophied prostate as in any other. It is only a question of delicate manipulation. If the hypertrohied prostate occurs in a man who has had instruments passed, he will have become habituated to them, and is therefore a better subject than

a healthy one who has not been so accustomed. Next, it was said that when the bladder could not empty its contents by its own power, and the urine had to be drawn off by means of a catheter, lithotrity was contraindicated, inasmuch as the fragments could not be passed. On the contrary, I rather prefer such a case, for the reason just assigned—that is, the bladder and the urethra are habituated to instruments; and as not to removing the fragments, there is no difficulty at all in removing the very last fragment, thanks to the improved methods now employed. If was said also that great irritability of the bladder was a reason why we could not crush. It was said that if the bladder could not hold above three or four or five ounces of urine there would be no room for the lithotrite to work, and therefore the surgeon must cut. I make no objection on that ground, because the irritation of the bladder is due to the presence of stone; and as soon as you begin to get away the stone the irritation diminishes. Besides, it is not necessary to have four ounces of water in the bladder: one ounce is ample. There is no occasion to have four or five ounces in order to perform the operation of lithotrity. It might have been so with the old, clumsy instrument; but with modern instruments there is no necessity to protect the bladder from contact with them. When instruments were used that were apt to catch the coats of the bladder it was no doubt desirable to have a quantity of water in the bladder; but with modern instruments, which will not lay hold of the coats of the bladder, there is no difficulty whatever in crushing with a single ounce of water. I do not care whether the bladder is empty or contains a large quantity of water, provided only that it is not too full. Nothing is worse than too much water, because the stone rolls about, and you must figuratively speaking, play a game of hide-and-seek to catch it. It is better to have an empty bladder than a bladder with half a pint of water in it.

You see then that the exceptions are very few; indeed there are very few adult cases which cannot be crushed, provided you give proper care and attention. If the present generation of surgeons now growing up, progress, as they must, and become more intelligent and more careful than those who have gone before—if they are better acquainted with the subject, as they must in the nature of things be, as our sons will be wiser than ourselves, and our grandsons wiser than they,—there will be fewer and fewer exceptions; because if the stone is discovered when sufficiently small, *it can always be crushed with an almost certain chance of success;* so that lithotomy must at some day disappear, except for cases which have been strangely neglected by the patients themselves, or have been overlooked by the medical attendant.—*Lancet, April* 25, 1868, *p.* 521.

59.—THE BEAKED SOUND.

By John Gay, Esq., London.

A reference to the comparative advantage of the modern over the old sound in the course of Sir Henry Thompson's lecture on stone, in this day's number of your journal, recalls my attention to the fact that in almost all works in which allusion is made to this instrument, its origination is accredited to those to whom, I believe, it does not justly belong.

The volume of your journal for, I think, 1845 contains a letter from me in which I detailed some observations which I had just then made on the bladder in the dead subject, for the purpose of showing what Sir Henry has forcibly reiterated—viz., that with the sound, *prior to that period*, it is impossible satisfactorily to search the bladder, inasmuch as the beak is too long to admit of that freedom of movement within the viscus that is essential to its complete exploration. After trying curves and beaks of varying sizes, I suggested the sound now in use, so far as its figure is concerned, and had a set made by Mr. Ferguson, of Giltspur-street, several of which I have still by me. I believe these were the first sounds that were ever made, or, at all events, used in this country, of the modern shape. The bulb at the end was a great improvement; but at whose suggestion it was made, I know not. It has been, I believe, accorded to Sir William Fergusson.

Had not the device of this "famous beak" been assigned to other surgeons *by name*, I should not have broken silence on the subject. As it is, I think I may be excused for seeking to give publicity to its true history, without incurring the charge of being over-nice about what some may deem "an inconsiderate trifle."—*Lancet, May* 2, 1868, *p.* 575.

60.—LITHOTOMY BY A SEMILUNAR EXTERNAL INCISION.

By Sir William Fergusson, F.R.S, Serjeant-Surgeon to the Queen.

[The operation of lithotomy, in consequence of the improvements in lithotrity, is now less frequently performed than formerly, but on this account it is not the less interesting. The interest in it indeed increases, for it is more dangerous, being confined to the worst cases, where lithotrity is inapplicable.]

Without discussing the applicability of the one or other of the two operations to the young, it may be affirmed as a maxim that between puberty and the age of fifty lithotrity will be the most suitable and the selected operation. Even above the last-named age a majority of cases will be most suitably treated by this

method; and if any between puberty and extreme old age be rejected or set aside for lithotomy, it must be for certain unfavourable conditions and indications which, if likely to increase the dangers of lithotrity, will most assuredly have a similar effect in lithotomy. In other terms, it may be said that all the cases of stone in the bladder in the adult, with complications, are now set aside for lithotomy; whilst all the simpler cases most promising of success are treated by lithotrity. Hence, then, it comes that the use of the knife in this disease seems more fatal than ever. A surgeon who keeps to lithotomy alone in the present day is hardly to be tolerated; he is behind the age, and his contempt or ignorance of lithotrity renders his opinion of little value. But the accomplished lithotritist knows and feels that he has in a certain number of cases to declare that some are not favourable for that mode of treatment, and these, still seeking relief from suffering, are consigned to the dangerous ordeal of the knife. It is true that some of these cases do well, and it has perhaps been fortunate that they have been rejected from lithotrity; but as a rule, there is great danger in meddling with them. That which may be bad for lithotrity is equally so for lithotomy. The common example of the stone being too large to be crushed is one where cutting and extracting are equally, even more, dangerous. If lithotrity is thought or found impracticable, the patient's life need not be placed in hazard; but lithotomy, once performed, sets him in a condition of jeopardy which may baffle the utmost skill.

These views have gradually acquired force in my mind, as experience in both operations has increased. Simple cases are selected for lithotrity—severe are set aside for lithotomy; hence the latter proceeding is more hazardous to life, and seemingly less successful than ever. That these conditions will acquire even greater prominence in time I feel strongly convinced; and as I am equally of opinion that, with our present knowledge, lithotomy will still remain a great and important agent in the hands of man to give relief from extreme suffering, it may be said to be more incumbent than ever upon the surgeon to endeavour to render the proceeding, if possible, less difficult and less dangerous.

[The oldest record we have of any lithotomy operations is that of Celsus, and it is a revival of this operation which is aimed at by Sir William Fergusson. The haphazard incisions of Celsus about the neck of the bladder are of course out of the question—it is to the external incisions alone to which reference is made. The medinal operation of Allarton is merely a modification of the Celsian, the means of dilating being alone somewhat different.]

From the time of Marianus to the present day the Celsian incisions have been referred to seemingly more for the sake of the curious than for the sake of instruction—more to deter than to attract. There is no evidence that Cheselden, who was perhaps one of the boldest experimenters among lithotomists, had ever tried the method ; and even Dupuytren's semilunar cutaneous incision was perhaps more to test the double-bladed lithotome than to show any feature of the operation of the Celsian date.

Of this I am sure, that throughout my own personal experience I have never heard a proposal to revive any of the steps of this most celebrated operation. Cutting on the gripe has been nearly obsolete from the time of Marianus, and the only instance I can think of as modern is that related by Samuel Cooper in his "Surgical Dictionary." With the all but universal neglect, if not condemnation, of the operation, no one seems to have drawn any distinction between the external lunated incision in the skin and the formidable wound in the neck of the bladder, devoid as much of anatomical knowledge as the gripe was of mechanical skill. The anatomist can scarcely contemplate the deep incision in the Celsian operation without a shudder ; yet if he considers the superficial wound, it may be difficult to point out a defect. My own impression regarding this is so strong that it constitutes the real object I have in view in offering these remarks.

In almost every instance in which I have observed difficulty in the operation of lithotomy, whether in cutting, in seizing the stone, or in extracting it, a short external incision has apparently been the cause. The cutting instrument has not been easily used in the deep part of the wound ; the point of the left forefinger has not been easily passed into the bladder ; the same with the forceps, and the blades have not been readily opened ; nor has it been possible to extract without swaying the wound in the skin as it has been stretched over the instrument and, in the last movements, over the stone. Much of the latter part of lithotomy is done with the forefinger of the left hand, and when the external incision is short its margins will appear to encircle the finger, so that there seems scarcely room to introduce the knife for the deep incisions. It is under such circumstances that the prostate is likely to be pushed before the point of the finger, and that the operator is likely to fail in reaching the bladder. When the perineum is deep, the further progress of the finger is arrested at the cleft between it and the middle one, and additional pressure merely pushes the skin towards the deeper parts. If it so happens, however, that the external incision is free, the finger readily passes deeper, so that the knuckle gets into the wound—a thing impossible in the short incision. When the finger and instruments can be easily passed

into the deep parts, the latter and nicer steps of lithotomy can be more readily accomplished, and therefore, in my opinion, with greater safety to the patient. Although fully aware that the operation has been most satisfactorily accomplished through an opening more like a stab or a round hole than an incision, I nevertheless consider the style very objectionable, and its happy results as exceptions to the general rule.

I have an impression that such palpable objection to a limited external incision had much influence in leading to a change from the Marian operation. It does not, however, appear that Jacques's incision at the side of the perineum was made with a view to length, but gradually, as the lateral operation became developed, a free opening in the skin in front, at the side, and below or behind the anus, was made an important feature; and the modern surgical anatomist has specially indicated that the knife may be very freely used between the anus and the tuberosity of the ischium to a considerable depth, as no important tissue lies in this locality. There is, however, a limit here to which the knife may be used with impunity, for the rectum, particularly if it be capacious, is in jeopardy if the incision be free and deep. Yet since Jacques's period, and particularly since the precise anatomical epoch of Cheselden, the lateral incision in the skin of the perineum has held its ground with but slight exception, and it may emphatically be called *the* mode of lithotomy of the present day.

Although I have practised the operation all my life, with a few exceptions, entirely in accordance with the views of Cheselden, I have from an early period had doubts if we were even yet on the right line for perfection. I have doubted if, after all, the side way has been the best either into or out of the bladder; and I have ever had in view the indications of Nature in this region. The contents of the bladder, of the vesiculæ seminales, although coming from the side, find their exit through the mesial line, and the *débris* from the bowels takes departure in like manner. But, above all, the ingress and egress to and from the uterus seem to me to indicate Nature's intentions as to a middle or central passage. The vagina must be allowed to be the *via sacra* between the urethra and rectum in the female; and although the analogy I mean to draw does not hold good as regards the ends of the vagina, or, in other words, the os uteri and the vulva, still I imagine that I may claim a propriety in the difference between the surgeon's operations and those of Nature, and at this distinction my lecture aims.

The grand immediate objects of lithotomy may be said to be an easy entrance to, and an easy exit from, the bladder, both being secured without unreasonable damage or risk to the patient. The old mesial or Marian operation seems to have

been so far destitute of these qualities as to have been in time utterly condemned and given up. With all deference and with all respect to such of my contemporaries as have practised or advocated the revival of this operation,—for I call it nothing else in its main features,—I make bold to say that the same fate must befal this recent innovation. From personal observation, and from what I have read and heard, I believe that more mishaps have occurred proportionately in this operation than by the ordinary lateral, with which all practical surgeons are unhappily too familiar.

The theory of the lateral operation is, upon the whole, excellent. The external wound is in a thoroughly safe line as far as regards damage to important structures; and the same may be said as regards the deeper passage to the bladder. The bulb of the urethra, and the rectum on the mesial line, the common pudic artery on the outer side, are intended to be thereby avoided; and the gliding between the accelerator urinæ and erector penis, without damage to either, seems a feat to gladden the heart of the physiological surgeon. True the transversus perinei must suffer, and so must some of the levator ani; but then, again, the division of the lateral lobe of the prostate in its long axis, about midway between the reflected portion of the pelvic fascia and the vesiculæ seminales, indicates that perfect precision in which the surgical anatomist delights. That such theory is generally realised in the lateral operation I thoroughly believe; yet I have my doubts if this really be the easiest and safest way to and from the bladder. Although I have followed the practice, as a rule, ever since I began to perform lithotomy —a period extending over thirty-five years—yet I have long been sceptical on this subject; and, as a proof of this, I may refer to my temporary adoption of Dr. Willis's proposal of lithectasy in 1842; and, again, to my proposal in that operation for an incision, the upper line to be in a part of the raphé, and the sloping lines on each side of the anus. Neither of these proposals seemed to take with the surgical mind in Britain; although the latter has been extensively and successfully used by Dr. Eve, of Nashville, as I have lately been told by that gentleman himself. Even with the experience I had acquired twenty years ago, I felt diffident about seeming to question the perfection of Cheselden's operation, and so I went on as best I could with the time-honoured lateral. The old feeling, however, cropped up again in me, and still leaning to the central part of the perineum, I ventured on another innovation. Instead of beginning the lateral incision on the left side of the raphé, I ventured more and more to the right, beginning a little to the right of the raphé, and then a little more; until, possibly, had I gone on, I might have tried a transverse wound between bulb

and anus, from tuberosity to tuberosity. I have been told that these views, good or bad though they may be, are not original —that this gentleman and that gentleman made such and such incisions. But I have failed to find any record of the practice, nor am I disposed to say much about it; although I have fully satisfied myself that crossing the border from one side of the raphé to the other was in no wise detrimental, the early impression on my mind (erroneous as it proved) being to that effect. But a direct transverse line did not satisfy my imagination so much as a longer oblique one, and at last I boldly ventured on doing what I had never seen or read of in modern surgery: I performed lateral lithotomy as regards the deep parts of the wound and prostate, beginning with the famous, but antiquated if not despised, semilunar incision of Celsus. On the 16th February, 1867, I cut a boy six years old, at King's College Hospital, in this manner. The operation was simple and easy; I may almost say, more so than is usual with myself. The recovery was perfect and without a bad symptom, although the wound healed more slowly than is common after the lateral operation at this age. On the 1st June following I had an opportunity of performing similarly on the adult—on a patient forty-eight years of age, in whom lithotrity had failed—on that account a case of extra danger, in accordance with the views set forth in the early part of this lecture. The result was highly satisfactory. The particulars of these two cases were published by the reporter for the Lancet in the number of that journal for July 13th, and since then I have had a third successful case in the hospital, on a patient aged 64. An example in private practice, on a patient aged 59, with most unpromising adjuncts, has also been crowned with a successful issue; and with this experience—one under puberty and three in the adult, with questionable data—I venture to bring this subject in this public manner before my professional brethren.

In all these cases the wound seemed to me to heal more slowly than in successful lateral lithotomy; but whether this was a coincidence in each, or the result of the peculiar line of incision, I cannot take it upon me to say. My impression is that it was only chance, but I should not quarrel with any who think otherwise, nor do I deem it a point of much importance, provided there be advantages otherwise.

It may be said that four successful cases are not sufficient to display any advantages to induce a trial of a new operation, and in some respects I willingly admit this; but it must be borne in mind that I write with an experience of nearly 200 lithotomies by my own hands, and it may be admitted that I should be allowed an opinion as to the peculiarities or features of different methods which I have myself tried. On this score

I can say unhesitatingly that I have been greatly pleased with the proceedings I refer to.

Although I speak of the Celsian operation, I beg it to be observed that this differs as essentially from that method as did the later operations of Jacques with those he first performed in Paris. When he began he cut at hap-hazard, and without a guide, into the bladder; but latterly, after being driven in ignominy from the capital of France owing to want of success, he used a guide— a staff as it is now called—into the bladder, and then it is supposed that his greatest triumphs came. The operations of Raw and Cheselden included this important feature, but it nowhere appears that there was any guide into the bladder in the Celsian operation, excepting the uncertain bulging of the stone as pulled and pressed downwards by the fingers of the surgeon introduced into the rectum. I have heard of Mr. So-and-So again, as in the oblique and transverse incisions, having made the Celsian incision in the skin with a staff in the bladder, but I know of no record of any such thing, and hence I am induced to call the attention of my brethren to the subject.

It may be said that Dupuytren's operation embodied all that I seem to intimate as new in this paper—the staff and the semi-lunar incision; and in some respects this may be admitted, but the double incision in the prostate and the use of the lithotome caché seem to have made little, if any, impression on the minds of modern lithotomists. Neither the double-bladed lithotome of this distinguished surgeon nor the single-bladed instrument of Frère Côme has taken with English surgeons, probably in consequence of the decided preference of the operation of Cheselden.

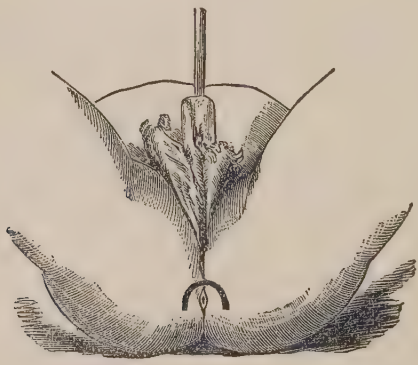
The proposal that I now make is more in accordance with that celebrated proceeding, and the chief difference is in the external incision. I have already referred to the seeming safety of that incision, and also to some of its dangers, but something more may be said on these points. When the perineum is presented in position for the commencement of lithotomy, the raphé usually appears as a hollow (excepting in very fat subjects) bounded on each side by the ischium. The external incision, being intended to run midway between that bone and the mesial line, is carried along the highest part of the surface—for the operator dare not go near the ischium, knowing the danger to the common pudic incurred thereby, and so he begins at the furthest distance or highest eminence from the bladder. In other words, it may be stated that a wound here, to reach the neck of the bladder, must be deeper than one in the middle of the perineum. The deeper the wound, the more difficult, and I may add the more dangerous, the operation; and, most

notable of all, the seizing and the extraction of the stone are more likely to be difficult through a deep wound than a shallow one, particularly when that wound or passage for the stone is limited to one side of the perineum. The objection which I have always felt to the Marian or mesial incision is that, being a straight line from over the bulb of the urethra to the front of the anus, it has been too limited to give facilities for the deeper part of the operation, and incapable of giving a fair and safe exit to a stone above average size. I admit, as a practised lithotomist, that skin and other tissues may be compressed or pushed towards the bladder in making way towards that organ, and again that when the stone is between the blades of the forceps the neck of the bladder may be drawn towards the skin, so as to make the perineum appear comparatively shallow; but I believe that both processes are highly objectionable in lithotomy, and the easier that the bladder is entered, and the easier that the stone is removed from it, the better. This leads me again to advert to the midway of Nature. When great space is required, as in egress of the head of the fœtus, the expansion takes place towards the ischia and the coccyx, and such should be the aim of the surgeon in dealing with a large stone. I am disposed to think that a semilunar incision facilitates this in a more efficient and satisfactory manner than any other. Here it might be said that the mesial incision carried into the rectum would fulfil this proposition, and so I believe it would; but all experience goes to show that such a wound of the sphincter and front of the rectum as this implies, is highly objectionable. There is no evil in the semilunar incision greater than that of the lateral; on the contrary, there seems to me less. For in this central locality there is in reality no vessel, nerve, or part to demand the surgeon's caution; the bulb and rectum are certainly in no greater danger, and assuredly the common pudic and its perineal or scrotal branches are in less. The great peculiarity of this semilunar incision, however, is that it gives such an extensive external opening beyond what in this locality any single straight line would. The gap opens something like the clasp of a purse's mouth; and the points of two or three fingers can be placed in it at once, so as to depress the anus and lower part of the rectum, and permit the knife to be freely used between the bowel and bulb and on the lower part of the triangular ligament. With sweep after sweep of the knife in the semilunar direction, a close proximity with the bladder will have been made; and now the operator may take his choice as to the steps of the future stages of the operation. Years ago I thought of using a blade of this kind after making an opening into the membranous portion of the urethra:—a sort of miniature double gorget, intended to be carried with its probe point along

a groove on the convexity of the staff, to notch both sides of the prostate ; but, being unwilling to change the cutting instrument, I resolved to try with one, and so, using the same blade which I employ in the ordinary lateral, to make the semilunar cut. I have at this stage aimed at the membranous portion of the urethra, and thereafter cut into the lateral left lobe of the prostate with the same blade in the usual way.

Thus I have combined the time-hallowed, or rather time-neglected, semilunar incision described by Celsus for cutting on the gripe and the modern lithotomy of Cheselden of cutting the left side of the prostate on a staff. All parties since the time of Jacques, who is said to have used in his latest operations a guide, director, or staff, to render his internal incision more precise, have agreed on the value of this great addition to lithotomy, and I need scarcely add that the staff is an essential feature in the operation to which I now draw attention.

The performance differs from the ordinary method only in the external incision, but a short description may be desired. The staff being introduced, the patient is to be bound and held in the ordinary way. The usual lithotomy knife, like a scalpel in a strong handle, is it to be held in the right hand with its cutting edge directly upwards ; the point is then to be introduced into the skin on the right side of the perineum, midway between



the anus and the tuberosity of the ischium and a little lower than the anus. It may be inserted a quarter or half an inch deep ; and, by a pushing or gliding movement upwards, the right side or end of the semilunar incision is made, then the curve, and then the slope on the left, so that a line like that

indicated here (see illustration) is completed. During the movement the knife is gradually turned round with the hand, whose radial margin, from being first turned downwards, is ultimately upwards. A second similar sweep, with the extreme ends not so deep, should next be made, when the fore and middle fingers of the left hand should be pushed into the wound between the bulb and rectum, with the intention of separating those parts; it will then be found that the anus and rectum can be depressed with peculiar facility. Now, too, with possibly a touch or two in the middle part of the wound, it will be found that the finger readily slips into the space between the erector penis and compressor urethræ; and the staff may be felt through the tissues between them and the membranous portion of the urethra. By a dexterous push, the point of the knife may be introduced into the groove, or perhaps a little further cutting may be used to divide a portion of the lower margin of the triangular ligament and part of the levator ani, or these may be divided in withdrawing the knife. The point of the blade should be pushed along, say, one-half of the membranous portion of the urethra, into the prostatic portion, so as to notch or cut freely the left lobe of this organ, or the latter may be done in withdrawal. Should the gorget be preferred, it may be used after the opening has been made in the membranous portion. The subsequent steps are the same as in the ordinary lateral operation, excepting that the forceps may be held more to the right, and withdrawn through the middle of the perineum, instead of the left side, as in the common way. Should the stone be large, the right side of the prostate can be more readily reached through this wound than through the lateral, and the good rule of pulling obliquely downwards in extracting can be more efficiently accomplished than through a wound in the side.

The after-treatment is in all respects as in the usual operation, a tube being used or not, in accordance with fancy or fashion. The skin in front of the perineum immediately behind the scrotum being left entire, the wound is scarcely observable as the patient lies on his back. In one of my cases a surgeon of experience on looking at the perineum would scarcely believe that lithotomy had been performed until he made a closer examination.

With the limited experience of only four cases, although three of them were adults, and comparatively unfavourable for operative interference, it would be wrong and beyond my wish to refer to this proceeding as specially superior to lateral lithotomy; yet I think it worth while to call the attention of my professional brethren to it from the conviction that it deserves trial, and with a hope that it may in some degree simplify, in

regard to performance and danger, one of the most formidable and interesting operations in surgery.—*Lancet, Jan.* 4, 1868, *p.* 1.

61.—ON LITHOTOMY BY A SEMILUNAR EXTERNAL INCISION.

By Sir Henry Thompson.

I venture to think that the following facts, very briefly related, will form a not unimportant contribution to the subject discussed in the interesting communications of Sir Wm. Fergusson and Mr. Erichsen.

Sir Wm. Fergusson has adopted in four cases of lithotomy the external incision of Dupuytren's bilateral operation, combined with the internal incision of the ordinary lateral operation.

Mr. Erichsen has adopted in one case the original operation of Dupuytren—that is, the external semilunar incision, with an internal one, similar in form and direction, made with the "lithotome caché," in both lobes of the prostate.

Now the most important point, it must be conceded, is the *internal* incision; that the external one, wherever it is, should be free, is agreed on all hands. Mr. Erichsen argues in favour of the division of both prostatic lobes, in order to obtain room for a large stone.

That incision I have now practised for several years, on these very grounds—viz., that the internal opening in median lithotomy is insufficient for any stone above a moderate size (and such are almost always crushed), and that the internal opening in lateral lithotomy is less safe than that made by the lithotome caché. The first time I used the lithotome caché for the bilateral incision of the prostate was November 19th, 1861, making the external incision in the median line, Mr. Erichsen kindly held the staff for me on that occasion.

During the last three years I have operated, *in the same manner*, on ten more adults, in all eleven cases, and of the respective ages of 43, 71, 73, 68, 80, 69, 55, 78, 45, 63, and 67 years. The three largest calculi so removed weighed respectively $3\frac{1}{4}$ oz., $2\frac{3}{4}$ oz., and $2\frac{1}{4}$ oz. The first and second of these three cases are still living, and were, like several of the others, cut in University College Hospital. It is true that any experience of eleven adults reckons for little in an estimate of mortality; still, it is the largest at present existing in this country, and the results with it, the ages considered, are satisfactory.—*Lancet, Jan.* 25, 1868, *p.* 132.

62.—ON MEDIO-BILATERAL LITHOTOMY.

By Vincent Jackson, Esq., Wolverhampton.

[The following article explains a plan adopted by Mr. Jackson for the removal of stone from the bladder of the adult in cases unfitted for crushing.]

On October 10th, 1867, I performed at Wolverhampton General Hospital lithotomy in two cases; one, a boy, cut laterally; and the other an elderly man, aged sixty-seven years, from whom I removed a large flat stone after the following method:—A large curved staff, deeply and widely grooved on the convexity, being introduced into the bladder, and the patient tied up, and placed in the usual lithotomy position, I inserted my finger into the rectum, and placing it firmly in front of the prostate, I entered a sharp-pointed straight-backed knife as near above the anus as the safety of the rectum permitted; and pushing it on to the groove of the staff, I freely divided the urethra, and as it was withdrawn I incised upwards in the median line the skin and superficial structures as extensively as prudence allowed. The left forefinger, now removed from the rectum, was passed into wound, and its point (nail downwards) lodged in the groove of the staff, sufficing as a guide to a broadish straight gorget, with a central projecting probe point, and a limited bilateral cutting edge. This was easily and gently pushed on into the bladder, incising as it advanced the prostate bilaterally from before backwards, and along its upper hollowed-out surface the finger slipped into that viscus. The former was now removed, and as small a pair of forceps as could be effectually used being introduced, the stone was seized and extracted.

The above operation I have had in my mind for many years (I practised it frequently in the dead-house of University College Hospital when I was house-surgeon, dissecting carefully the parts afterwards, and I have also in my possession some recent dissections); and although I have had many opportunites, I never resorted to it previously, having been satisfied hitherto with the lateral method in all cutting operations. But the above case being in many ways a very unfavourable one for operative interference, cystitis to a considerable extent existing, and the man's health being completely broken down with a long life of drinking, and of late years with much suffering, I resorted to it, trusting to reduce the risk to a minimum, and also bearing in mind the greater facility which, in my opinion, the median section of the perineum offers to arrest venous bleeding, if it occurs, by a more complete and perfect plugging of the wound, either around a tube or upon a catheter; and in this particular case I anticipated some trouble from this danger.

In conclusion, I would remark that, however excellent the architect, and however good the workmanship, time and accumulated experience must settle the worth of a thing; so we have yet to look to the future to determine the question whether a better or a safer operation can be devised than the old lateral method of Cheselden.—*Lancet, March* 7, 1868, *p.* 330.

63.—MEDIAN LITHOTOMY WITH BILATERAL SECTION OF THE PROSTATE.

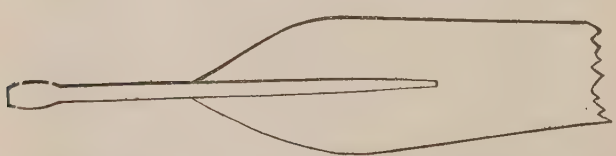
By Jonathan Hutchinson, Esq.

[On reading the preceding paper by Mr. Vincent Jackson Mr. Hutchinson discovered that he had been working in the same direction as Mr. Jackson with regard to improvements in lithotomy instruments.]

During the last ten years I have had made, and have repeatedly used, both on the dead and the living, various modifications of the double-cutting gorget, with probe-point, and designed for use with a median incision. The last which I have had made is, I should suppose, almost exactly like that contrived by Mr. Jackson, and I am very glad to read his testimony as to its easy use and the neatness of the section of the prostate which it makes. I believe it will prove a very valuable instrument, and I was only waiting for further experience before bringing it under the notice of the profession.

Although our knives, or rather gorgets, are, I expect, alike, and although we both propose a median external incision, yet I am sanguine that I have got a little further than Mr. Jackson in one or two points. My staff is rectangular, and at the end of the groove is a little thimble to receive and retain the probe-

Fig. 1.



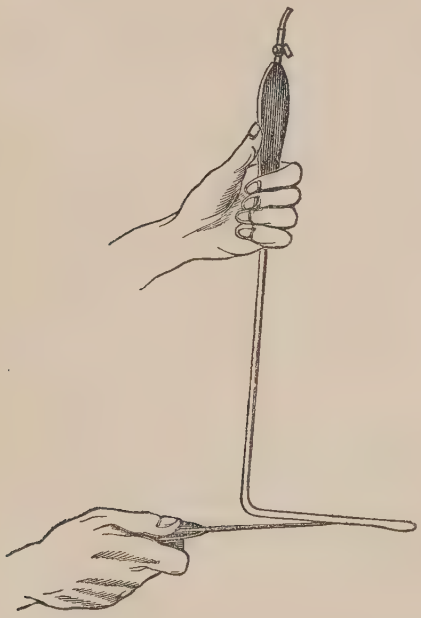
point of the gorget. Instead of making the gorget move on a staff which is fixed, I pass the probe end of the former forwards into the thimble, and having thus locked both firmly together I then take the staff from my assistant, and pass both together forwards and upwards into the bladder. Thus the great danger of the cutting gorget, that it may slip from the staff, is obviated.

With a rectangular staff the whole of the groove is quite straight, and the probe finds its way in much more easily than on a curve. There is not the slightest difficulty in introducing it.

The accompanying woodcuts will explain my instruments.

In Fig. 1 the flat surface of the gorget is shown, and the proportionate length of its probe end. It is taken from one made for me by Krohne about a year ago.

FIG. 2.



In Fig. 2 the gorget and staff are seen together as they would be just prior to the final thrust. The dotted line indicates the part where the probe joins the blade. The whole of the probe is concealed in the thimble and groove of the staff. The blade is of course seen edgewise.

The advantages of the proposed method are: 1. Median incision of external parts, and diminished risk of hemorrhage. 2. Ease, certainty, and celerity of performance. 3. A bilateral

section of the prostate of determinate size, certain to afford plenty of room, and yet certain not to wound the capsule. 4. The certainty that no injury will be inflicted on the important parts in the floor of the prostatic urethra.

The merit of the invention of the rectangular staff, of course, rests with Mr. Buchanan, of Glasgow. He, however, proposed its employment for a special kind of operation. I have never myself performed his operation, but I think very highly of the convenience of the rectangular form of staff. Mr. Buchanan thought the instrument not suited for the ordinary lateral operation, and deprecated in strong terms its use for that purpose. I have not found his prognostications verified. I believe I have performed upwards of forty lateral lithotomies on a rectangular catheter staff, and certainly have no intention to go back to the curved one.

Whilst on this subject, may I be permitted to express my conviction as to the extreme importance of the use of a tube after the operation. If this were general, I believe it would prevent many deaths. Especially is it valuable in the adult, in whom the long, deep wound causes risk of infiltration of urine. The tube which I use was described in the columns of the Lancet about eighteen months ago. It consists of a large silver canula, curved at the end. Through its whole length is passed a continuous piece of sponge, which, projecting into the bladder, soaks up the urine, and conveys it away with a degree of certainty which a mere tube would not afford. Instead of silver, an india-rubber tube, of a quarter-inch bore, might be used, with, of course, the sponge passed throughout, and projecting well at each end. The outer end of the tube may be received into an india-rubber bag or larger tube, and thus the patient's bed kept dry during the first few days. Of my last eight lithotomies *in the adult* (most of them in old men), all but one have made good recoveries; and I attribute the success very largely to the use of the tube. The man who died was believed to be the subject of diseased kidneys at the time of the operation. — *Lancet, March* 21, 1868, *p.* 391.

64.—ON LITHOTOMY BY A SEMILUNAR EXTERNAL INCISION.

By John E. Erichsen, Esq., Senior Surgeon to University College Hospital.

[Mr. Erichsen agrees in the main with all that is contained in the preceding paper by Sir W. Fergusson. As, last summer, he removed a large calculus through a semilunar external incision, he reports the case, now that an interest in the subject has been excited by Sir W. Fergusson's paper.]

The operation was performed on the 7th of August. The patient having been placed under the influence of chloroform, Mr. Erichsen passed a grooved rectangular staff into the bladder; and the stone being at once felt, he proceeded to perform the operation after the bilateral fashion. A transverse crescentic incision was made in the perineum. The centre of the incision was half an inch above the anus, and each extremity of it about the same distance from the tuber ischii. The dissection was carried down in the line of the superficial incision to the central point of the perineum, so as to separate the bulb from the rectum. The scalpel was then thrust onwards into the groove of the staff, through the membranous urethra, and just in front of the prostate, its blade being directed upwards. The knife was now drawn backwards out of the wound, cutting upwards as it was withdrawn so as to form a vertical incision in the superficial structure, of about an inch in extent, in the middle line, and communicating with the centre of the crescentic cut originally made. The knife being withdrawn, a lithotome caché was introduced along the groove of the staff; and by the withdrawal of this (the handle of the instrument being at the same time depressed) both lateral lobes of the prostate were divided to the extent of about three quarters of an inch—the distance to which the instrument had been previously adjusted. The lithotomy forceps was then introduced into the bladder, and the stone was at once seized. The withdrawal of the forceps was effected with some little difficulty, owing to the large size of the stone, and to the necessity of changing the direction of its axis in their blades. The calculus on removal was found to be of an oval form, with a long diameter of about two inches and three quarters, and nearly two inches in a transverse direction, and from its surface several irregular nodules projected. It weighed three ounces and one drachm. Its external surface was composed of uric acid. A tube was passed into the wound, and left for about forty hours, the urine coming away freely through it.

Aug. 10. The report continues as follows; Pulse 84; the patient going on well. From this time he progressed uninterruptedly (with the exception of a slight attack of diarrhœa, which was easily checked,) taking his food well, sleeping well, and suffering no pain whatever.

On the 22nd the urine ceased to flow through the wound, and came entirely by the penis.

The wound granulated healthily, and, occasionally being touched with nitrate of silver, was almost entirely healed when he left the hospital on the 11th of September.

It will be seen by a reference to this case that I performed the operation by Dupuytren's method. This plan, as is well known, is the original "bilateral" operation by means of the "lithotome

caché," a most beautiful and ingenious instrument devised expressly for it. Civiale modified this operation by making the external cut perpendicular in the mesial line, but retaining the double section of the prostate; and Sir W. Fergusson has still further modified it by confining the section in ordinary cases to the left lateral lobe of the prostate.

The principle of all these three operations appears to be the same—namely, to reach the neck of the bladder through the mesial line, where it is nearest to the surface. The difference in the details is, however, great, and I think most important.

So far as *external* incision is concerned, Sir W. Fergusson adopts Dupuytren's method—the crescentric transverse; thus getting wide space, enabling the rectum to be fairly separated from the bulb and well depressed, and cutting below or rather behind the arteries of the perineum. In all these respects I think this incision is preferable to the perpendicular external cut of Civiale and Allarton.

In the internal incision—that through the prostate and neck of the bladder—Civiale nearly follows Dupuytren, making the cut equally into both lateral lobes by means of a lithotome caché; whilst Sir William confines his cut, except in certain cases, to the left lateral lobe, and makes it with the scalpel.

To my mind the double cut appears the preferable method, as by it an equal extent of incision can be made in the prostate with less danger of passing beyond its limit. Suppose, for the sake of argument, that to extract a calculus it is desirable to make a cut eight lines long into the prostate. If one lobe only is incised, the cut must go to the full extent in it. If both lobes are cut, only four lines need be divided in each of them; and so proportionately to any extent. As the whole under-surface of the prostate is exposed by the transverse external cut, it is as easy to divide both lobes as one. This may be done with the scalpel, and, in such dexterous hands as those of Sir William Fergusson, with perfect safety; but for a less expert master of our art I think the lithotome caché a safer instrument, as by it the extent of incision can, without possibility of error, be regulated to a hair's breadth.

There is one practical point in all these mesial operations of lithotomy—whether Allarton's, Civiale's, or Dupuytren's—which I consider of some little importance. It is the use of the rectangular in preference to the ordinary curved staff. I have now used it several times in Allarton's, and Civiale's, and look upon it as possessing three very decided advantages over the curved staff. The first is, that its angle can be placed directly in the membranous part of the urethra, and held there until the knife enters its groove. Thus it becomes an unerring guide to the exact part of the urinary canal that we wish to open.

The second advantage is, that it carries the urethra away from, instead of as the curved staff does, down against the rectum. And the third is, that from the angle onwards the course of the groove is straight and direct, so that the beak of the lithotome or the point of the scalpel is carried on in a straight instead of a curved direction.—*Lancet, Jan.* 11, 1868, *p.* 41.

65.—ON LITHOTOMY BY A SEMILUNAR EXTERNAL INCISION.

By Dr. J. Ogden Fletcher, Manchester.

[The following is elicited by Sir Wm. Fergusson's paper.]

During the years 1846-8, the late J. A. Ransome, Esq., very constantly operated, at the Manchester Infirmary, in the way suggested by Sir W. Fergusson, with the trifling difference of extending the left crus of the semilunar incision about two lines lower down, between the rectum and ischium on the left side, "to allow the urine a more easy escape."

The reasons given by Mr. Ransome for adopting this combination of the two acknowledged operations were those so well set forth by Sir W. Fergusson. I have notes of more than twelve cases operated on in this way by Mr. Ransome with very good results. And if my memory serves me well, Mr. Jordan has many times operated in this way; but, as I have not any notes of his cases, this I give from memory alone.—*Lancet, Jan.* 8, 1868, *p.* 105.

66.—ON THE EXTERNAL USE OF DIGITALIS IN SUPPRESSION OF URINE.

By J. D. Brown, Esq., Haverfordwest.

Case 1.—Mr. H., a healthy young farmer, aged 23, was suddenly seized with severe pain in the bowels and back. I saw him at the end of seven days. Bold treatment had been enforced by Dr. John Thomas, of Narberth, such as bleeding, sweating, blistering, warm baths, but in vain. A catheter was passed on my arrival, and about a teaspoonful of urine was removed, highly albuminous. His condition was nearly hopeless when we met the next day. Vomiting and nausea prevailed, with heavy dull pains, and he was evidently sinking. It was now the ninth day, when we agreed to try the effects of digitalis, It was useless to administer it by mouth, and there was no time to lose. It was the month of May, and digitalis was plentiful. A poultice of leaves, bruised and warmed in boiling water, was applied at 12 a.m. We left, saying that if no urine came away by six in the evening a fresh poultice was to be applied. We had no sort of hope of the patient's recovery, and communicated

our opinion to the friends on leaving. By 6 p.m. no urine. A fresh poultice was applied to the abdomen. About 10 that night urine passed. At 4 a.m. I was sent for, and Mr. Thomas, who lived nearest, got to the house by 6 or 7. The messenger, to my utter surprise, said that they could not stop him making water. He had then made eight ordinary-sized chamber vessels full, and was still making it when he left. We met at 10 a.m., but he never rallied; the drain was too much. Digitalis had been badly handled by us. We left no guide, no rule, and too much was absorbed. It did well, but went beyond its work. He lived till night.

Case 2.—A. R., servant, aged 45, subject of renal calculus and gout, was suddenly seized with suppression, but had no great amount of pain beyond what he usually suffered in passing small stones. I saw him on the fourth day, in consultation. All the usual remedies had been tried in vain. It was winter, and digitalis was given, but not in bold and sufficient doses at first. At the end of the sixth day it was boldly given in large doses, and a poultice applied on the seventh day; urine passed freely, and all went well. He still lives in his usual health.

Case 3.—Mrs. L., age 50, subject of renal calculus. Suppression came on, with vomiting and the usual symptoms. At the end of four days, every other plan having been tried, I recommended the poultice. It was winter, but leaves were obtained and applied as usual. I returned in six hours, when two chamber vessels full of urine had been passed. All the symptoms gave way, and for two days she continued to do well. Again suppression came on; she was neglected by her attendants, and the poultice was not again applied. She sank at the end of the twelfth day, no water being passed except for the three days when digitalis was in the field.

Case 4.—Mr. G., a gentleman about 40, subject of renal calculus, having passed one year ago, in good health up to Friday morning, when he was suddenly seized whilst dressing with intense agony in the right renal region. His medical attendant, Dr. Rowlands, of Carmarthen, saw him immediately, subdued the pains, but, to his surprise and vexation, no urine passed. A catheter was passed; there was no urine in the bladder. The usual treatment (baths, leeches, and opium) was actively employed without avail. Dr. Lewis, Carmarthen, was then called, who persisted, in conjuction with Dr. Rowlands, with the remedies. On Monday, I saw him with Dr. Lewis. I told him of my success with digitalis; he was surprised, but did not like to undertake its management unless in concert with Dr. Rowlands, who was then out of town. We agree to give it in one-grain doses every four hours until Tuesday morning,

when we again met. We then agreed to apply the poultice. It was May, and we procured plenty of fresh leaves. Dr. Rowlands and myself made and applied it. He undertook to watch the pulse, which we took for our guide. It was 109 at 11.30; catheter passed no urine; few drops of blood; poultice applied over abdomen; in ninety minutes, pulse 75; poultice taken off. A call for the chamber vessel; a good stream of water; in twenty-two hours, seventy-five ounces were passed. Specific gravity 1·15. Acid, slightly albuminous, a little blood, and casts. Improvement kept pace. Friday: Symptoms of stone again, which gave way to the usual treatment. Saturday: Plenty of urine; recovery complete.

Case 5.—Mr. R., aged 54, subject of renal calculi, was suddenly seized with symptoms of renal calculi passing off in December, 1867. Mr. Hicks, of St. David's, and Mr. Howell saw him, and used every available means that skill and practice could command for two days, but in vain. Mr. G., of Carmarthen, the subject of the preceding case, being in the neighbourhood, hastened to the house, told the doctors of his cure, and begged them to try it immediately and to send for me. Fresh leaves were collected, and a poultice applied, but having no guide nor experience in its use, it was taken off too soon. I arrived at 4 a.m. Being sixteen miles distant, much time was lost. This was the third day. A catheter was passed, to satisfy ourselves as to state of the bladder. No urine. I had brought some dried leaves and tincture with me. A poultice was made of ℥ss of tincture, with fresh and dried leaves; poultice applied at 5 a.m.; pulse about 80. At 8.30 a.m.: Pulse reduced about 15, and about ten ounces of pale, clear, slightly albuminous urine came off. A fresh poultice applied at 10, and I left, with directions to take it off when the pulse came down to 60. Plenty of urine continued to be secreted, and from that time he has continued to improve. Urine is now slightly albuminous and alkaline, in spite of our remedies. This gentleman was much out of health, and had been for two years the subject of renal calculi, nausea, and dyspepsia, but he is much better, and improving under phosphoric acid and iron.

Case 6.—Mrs. ——, aged about 40, was suddenly seized with suppression of urine. In spite of every remedy, to the fourth day the suppression continued. Severe symptoms were now developed, and a tendency to stupor was amongst them, pulse indicating mischief. It was now decided that they should try the digitalis plan. One ounce of the tincture was mixed with a warm linseed poultice, and kept on overnight. The next morning a large quantity of urine passed. The poultice was kept on the abdomen all next day; urine was plentifully secreted; and from that time she improved and got well.

There may be a difficulty in obtaining leaves collected before seeding time, which appears to me to be the period of most vigour, judging from the fact that Nature concentrates her powers for the multiplication of species, and which at seeding time would pass into the young, leaving all other parts more or less exhausted. I believe from experience the winter leaf is very deficient in power. The tincture, mixed with linseed meal, succeeded in Messrs. Rowe and Phillips's case (Case 6); the fresh powder or dried leaves would be equally efficient. The rules of management must depend on the pulse. I have seen no good results till the pulse fell in number; it matters not from what figure: fall it must before any change occurs. In Mr. G.'s case it fell from 109 to 70 in ninety minutes; in Mr. R.'s, from 80 to 65 in three hours and a half. I would strongly advise 60 as a standard from a high number; 40 or 50 from a lower figure—say from 80. Judging from the effects on the circulation, we cannot lose sight of the fact that the arrest of secretion depends on capillary congestion, which in turn might, by pressure, paralyse the nerves. The fact, however, remains that we compel the kidney to resume its functions by diminishing the force of the circulation, lessening the quantity of blood by allowing a much longer interval between each new arrival. Strange, too, it is that in four cases the attack commenced suddenly like a fit of stone, and, in reality, stone came away in each case.

These cases are reported with the object of calling attention to the effects of digitalis in that dangerous disease, and of inducing the medical world to give it a trial. It is not supposed that it will succeed in all cases of that mysterious disease; but it is clear that it has a powerful influence over the renal secretions, and if carefully watched, taking the pulse as a guide, no mischief need be feared. I own there was a want of caution in the management of my first cases—indeed, it was only used as a hopeless remedy—but as the truth broke upon me, it was clearly seen there was a power in use I had not clearly foreseen, and more skill and caution was enforced in its management.—*Medical Times and Gazette, Jan.* 25, 1868, *p.* 86.

DISEASES OF THE EYE AND EAR.

67.—ON A NEW METHOD OF EXTRACTING IN CASES OF CATARACT.

By Dr. Charles Taylor, Honorary Surgeon to the Nottingham and Midland Eye Infirmary.

[The writer states that he was induced to practice the mode of operating recommended in the following paper owing to his being

dissatisfied with the results of some cases in which the lens had been extracted by Schufte's method. There is such a feeling of utter powerlessness when any accident occurs during the performance of the ordinary flap operation. The point aimed at by Dr. Taylor is to extract through a wound so small, that there is no risk in such manipulation of the eyeball, as are necessary to facilitate the various steps of the operation, and yet large enough to permit the exit of the lens without the introduction of any instrument into the eye, or consequent risk of those disastrous results which sometimes follow the lifting out of the lens with Schufte's spoons.]

I propose to call this mode of operating "Extraction by Linear Flap," as I think the term expresses its peculiarities better than any other, and will serve to distinguish it from Von Gräfe's strictly linear method, more especially in those points in which it essentially differs, and, in my opinion, possesses some advantages. I must mention that I had fully worked out this method, adopted it in my practice, and demonstrated its advantages to my medical friends, before I had heard of Prof. Von Gräfe's modification of linear extraction, and before any account of his experiments had been published. On first hearing of his modified linear extraction, I was under the impression that the two operations were almost identical; but having seen Von Gräfe's operation, I find material differences, although I have no doubt from what I have since learned, that we arrived at the same conclusions as to the necessity for a modification of Schufte's and the flap operation by a similar process of reasoning, and apparently at about the same time. In my first operations, as the most convenient instrument accessible in a provincial town, I made the incision with a ground-down Zehender's cataract knife, reduced to about the size of the ordinary secondary cataract knives sold in most cases of ophthalmic instruments. I have since used Von Gräfe's knife; but it is quite possible to make the incision with Bader's smallest sized iridectomy knife, cutting gently round from left to right within the prescribed limits. As the risk, even if vomiting occurs, is very slight with this form of incision, I usually prefer to have the patient narcotized. Keeping the lids apart with the stop-speculum, I then fix the eye by seizing the upper border of the external rectus tendon with a pair of forceps, and enter the long narrow-bladed knife in the corneo-sclerotic junction, about the commencement of the upper third, or junction of the upper with the middle third of the cornea, say $2\frac{1}{2}$ or 3 lines lower down than in Von Gräfe's method, and rather more forward in the corneo-sclerotic junction. The site of the entrance puncture must vary somewhat, according to the size of the globe, as a

very shallow flap will suffice if the cornea is of average or more than average dimensions, and a deeper one will be required if it is below the standard size. Care must be taken, in entering the knife, that it is made to penetrate the chamber at once. Any gliding between the layers of the cornea is much to be deprecated, as the consequent want of correspondence between the internal and external wound will be sure to cause disappointment in the subsequent steps of the operation, and the lens will be impeded in its exit by little bands of partially incised corneal tissue occupying the angles of the wound. Having entered the chamber, the knife must be pushed gently across, and made to emerge at a counter puncture similarly placed. The necessity for using a knife very small in bulk, narrow, and sharp-pointed, will here be appreciated, as the resistance offered by the dense tissue to the passage of the knife in this situation is often sufficient to cause slight bending of the blade. It is important that the counter puncture should not be made too far back—an accident very apt to occur, and by which one is betrayed into making an unduly large wound, a considerable portion of which will be situated in the sclerotic, thus giving rise to risk from escape of vitreous humour. A little practice, the avoidance of haste, and occasional apposition of the operator's forefinger, will usually suffice to prevent any trouble on this score. Having completed the counter puncture, I push the knife well through, and, holding it parallel to the iris, cut directly upwards about two lines and a half, or until the edge of the blade is on a level with the central summit of the cornea. It is then turned directly forwards, and the section completed by a gentle sawing movement, thus forming a small, shallow, elongated flap, situated well back in dense tissue, with sloping sides and transverse centre, which last occupies the summit of the true cornea. The aqueous escapes when the counter puncture is made, and unless a preliminary iridectomy has been performed, the iris frequently folds over the edge of the knife. Gentle pressure on the cornea, and careful management of the blade, I have always found sufficient to free it, and enable the operator to complete the section without wounding the iris.

Having completed the incision, I usually remove the speculum so as to allow the eye to recover somewhat, and a little aqueous humour to collect, and then preceed to the second step of the operation. It is safer now and subsequently to insert only one blade of the speculum under the upper lid, the other blade being supported on the patient's cheek by the finger of an assistant; scarcely any pressure is thus made on the eyeball, and risk of rupture of the hyaloid membrane is considerably diminished.

As the conjunctiva is only incised, and to a very slight extent at the extremities of the wound, there is usually little trouble from hemorrhage up to this stage; should the incision, however, by accident be placed too far back, and bleeding occur, it is well to apply slight pressure to the eyeball with a pad of cotton-wool, before proceeding to the second step of the operation, which, if an iridectomy has not been premised, will be the excision of a small piece of iris. This is readily accomplished by inserting the forceps into the central portion of the incision, which occupies the apex of the cornea, while an assistant, if necessary, gently depresses the globe. This situation of the central portion of the incision enables the operator to enter the chamber at once, and the difficulties sometimes met with when the forceps and pricker have to be thrust under loose conjunctiva, with its sub-mucous tissue infiltrated with blood and aqueous humour (a source of embarrassment in Von Gräfe's method) are thus avoided. Having introduced the forceps, the blades must be opened and the iris seized near to the pupillary border, gently withdrawn, and divided close to the edge of the section. For this purpose I use very fine-pointed bent scissors, and insert the points just within the angle of the incision; if this is not attended to, the iris is very apt to prolapse in this situation, and to some extent delay the progress of the case as well as occasion a little deformity, which will be noticed afterwards. Having divided the iris at the right angle of the incision, it must be separated gently, so far as may be, from its ciliary border to the opposite edge of the wound, and cut off with the same precautions to secure the small piece so apt to prolapse as before. Should the iris bleed, so as to threaten to fill the anterior chamber, and so obscure the future steps of the operation, it is well to repeat slight pressure with a cotton-wool pad for a minute or two; this will arrest the hemorrhage, and any blood may then be removed by suction, or coaxed out by manipulation of the eyeball, while the posterior lip of the incision is gently pressed back with the curette. In one case (Mrs. N., Case 44), where it was very desirable to enlarge the pupil, I excised a portion of the lower central segment of the iris through the same wound, and at the same time as the upper, with a very good result. The iridectomy is more readily performed, and the excision of a smaller portion of iris will suffice, if the pupil is well dilated prior to the operation. This is best effected by a strong neutral solution of atropine, applied the night before, and repeated in the morning. The pupil contracts on the escape of the aqueous humour, but not so readily as it would, if not previously dilated; and the subsequent dilatation on which the success of the operation may in some degree depend, is afterwards much more easily maintained. I have, in several cases,

where great age, marasmus, arterial degeneration, or the existence of other causes was likely to impede the progress of the case, or otherwise vitiate the result, adopted Mooren's precaution, and performed the iridectomy six weeks before extracting, and in very unfavourable cases, as in the eye remaining to be operated on in Smith's case, where suppuration threatened for twenty-four hours after the extraction. I have excised a large piece of iris both from the upper and lower segment of the pupil as a preliminary precaution. This division of the operation of extraction for cataract into two portions, with a prolonged interval, is, however, manifestly inconvenient both for the poor, who can ill spare the time required for two operations, and also for the wealthy, who often have a considerable distance to travel. Sometimes, also, from a want of correspondence between the wound made in extraction and the smaller one used in the iridectomy, I have found the iris more apt to prolapse at the corners than if excised in proportion to the length of the incision at the time of extraction. In some cases, however, a preliminary iridectomy has many advantages; by it the operator becomes thoroughly acquainted with his patient, and he is enabled to form a correct opinion as to his behaviour under extraction—a matter of no slight importance, as all accustomed to deal with such cases must allow. The important question as to the use or omission of chloroform may also then be decided; and, if administered, its effect upon the patient at the time, and subsequently, so far as vomiting is concerned, may be noted and utilized to the evident advantage of both patient and operator. In many cases also, where cataract is complicated with other manifest or incipient diseases of the eyeball, tension is reduced, and the tendency to cyclitis diminished by a preliminary iridectomy; subsequent extraction is easier to perform, and risk from bruising the iris abolished. The operation is thus reduced to its simplest elements, and chances of failure materially lessened by eliminating causes of accident. Having completed the iridectomy, the operator should seize the globe with forceps close to the cornea, above the insertion of the inferior rectus tendon, depress the globe with very slight pressure, and incise the capsule freely, taking care to pass the pricker beneath the lower segment of iris, and open the capsule from bottom to top, in two or three places. In withdrawing the pricker, it should be gently inserted under one of the angles of iris left by the excision, and drawn to the extremity of the other, so as to lay the capsule freely open, on a level with the equator of the lens. It is important to open the capsule in this situation, as I have more than once seen the exit of the lens retarded by little undivided shreds of membrane occupying the angles of the wound. Not unfrequently the lens comes forward, and is

extruded immediately the capsule is incised; but, if from any cause it should be retarded, it will be necessary to make pressure on the globe, while the posterior lip of the wound is slightly pressed back with a small spoon or curette. Pressure is best made with the ordinary strabismus hook, placed in contact with the globe, below the insertion of the inferior rectus tendon, about four or five lines from the margin of the cornea. It must be moderated according to circumstances, should cease when the lens is partially extruded, and evacuation completed either by the insertion of a sharp hook into the lens, or by gentle pressure on the corneal surface, so as to lift forward its lower border as it is in the act of escaping. Cortical fragments, if any remain, may usually be extruded by slight pressure; but if this does not suffice, I usually place a temporary pad of cotton-wool on the eye, and allow the patient to recover from the chloroform narcosis, when the aqueous will be re-secreted, and any fragments concealed by the iris may be washed into the pupillary area by slight pressure on the globe external to the lids, and evacuated by gently opening the wound with the curette. If any remain, I remove them with Mr. Teale's suction curette (the sucker must always be used with great caution, otherwise hyperæmia ex vacuo will be induced), and then tear across or withdraw any little fragments of capsule with iris or canula forceps. By thus carefully removing the capsule subsequent to the extraction of the lens, I believe all the advantages obtainable by the evacuation of the lens, with its capsule entire, as in Pagenlecher's operation, are obtained; but if the operator should desire to remove the lens and capsule in its entirety, as in that operation, I believe the form of incision which I have recommended is the one best adapted to secure a safe and perfect result. Should the lens, from adhesions or other causes, resist such pressure as may be safely employed without the risk of rupturing the hyaloid membrane, as has happened in some of my cases, I extract it with a small skeleton spoon. From the large size of the wound this becomes very easy, and as the flap yields readily, the lens is easily tilted forwards, and there is no necessity to incur the risks inseparable from the insertion of the spoon too far back into the vitreous cavity.

If it is desirable to extract by the lower section, the same form of incision may be employed. The operator seizes the conjunctiva above the cornea in its vertical meridian with toothed forceps, while an assistant draws down the lower lid, and assists in fixing the globe. The lower section is preferable in cases where the lens is superficially softened, as any soft cortical matter left behind in the eye will gravitate to the bottom of the anterior chamber, pass harmlessly through the coloboma iridis, and either undergo absorption, or be washed out in the

gushes of aqueous humour. The risk of its lodging behind the lower segment of iris and setting up subsequent irritation is thus avoided, but the section is less sheltered, and the coloboma uglier and more disturbing to vision.

The operation completed, I apply a drop of solution of atropine in the corner of the eye, and cover the lids with a pad of fine cotton-wool dipped in, but not saturated with, cold water. If sodden, the pad is heavy, irregular, and unmanageable ; but just moistened on the surface, it fits accurately into the orbital cavity, and speedily dries into a thin smooth film closely bedded to the surface of the lids, absorbing moisture, insuring equable support, a regular temperature, and the exclusion of light. It must be secured *in situ*, with gentle pressure, by thin strips of soap plaster, spread on unglazed calico (which adheres without warming). I find this by far the best dressing. Pressure can be accurately applied by strips fixed to the forehead and cheek, and, if necessary, by an additional broad strap passed from temple to temple. I apply atropine and compress to both eyes, with a view of preventing the rolling of the eyeball and sympathetic action of the pupil, which takes place in concert with its fellow if one eye is uncovered. Over the compress I usually fit a strong pasteboard shade on to the forehead, with a little wool intervening, and extending to the tip of the nose. This will protect the eye from any accidental blow or involuntary motion occurring during the sleep of the patient. Any kind of compress bandage, if required, may be readily applied over the pads and plaster. If there should be much pain complained of after the operation, and it should not cease in a few hours, I give a small dose of morphia, and repeat it if necessary. Should this not suffice to relieve the patient, I apply four leeches to the temple, and as fast as one drops off replace it with another, until either the symptoms are relieved, or it is evident that no benefit may be expected from further depletion. Occasionally, the injection under the skin of the lid of $\frac{1}{6}$ to $\frac{1}{4}$ of a grain of morphia will afford immediate relief ; and if there is much heat complained of, or perceptible increase of temperature, a linen compress must be applied, wet with iced water, and kept cold by a small piece of ice placed in the corner of the orbit, and re-applied as fast as it dissolves. A brisk purge is sometimes of service, and blood, in cases favourable for depletion, may be taken from the arm. Other symptoms must be treated as they arise on general principles. If swelling of the lids comes on, with profuse semi-purulent secretion, the compress bandage, as advised by Von Gräfe, must be applied, and changed every four hours, a hot aromatic poultice being applied for twenty minutes at each change.

The swelling of the lids may also be treated with a solution

of nitrate of silver, in equal parts of spirit of nitric ether and distilled water (10 gr. to ʒj.), or by the application of tincture of iodine. Chronic irritation may be relieved by similar means —mercurial inunction with belladonna over the brow, and the evacuation of the aqueous humour, prolonged rest in the recumbent posture, or a second iridectomy downwards. If any capsule is left, it is better not to attempt its removal for two or three months. As to results, since devising the above described method, I have operated on 52 cases of cataract in succession, with only one complete failure.

The ages of the patients range from 5 months to 86 years. The majority, however, were persons of great age, several verging on 80, six being upwards of that age. and four upwards of 85 years. Some were suffering from marasmus and constitutional cachexiæ, others from various diseases of the eyeball, tending to vitiate the results of an operation. These persons were operated upon in succession as they applied, no selection was made, and I have never refused a case. Indeed, two or three, including the one complete failure (occurring in a feeble woman, aged 86, blind for twelve years with degenerated cataract and firm synechiæ), had been declined by other surgeons. In two cases (aged 65, No. 23, and aged 68, No. 17) healing was protracted, and the patients remained a month under treatment. One (aged 83, Case 18) died from apoplexy, the result of epilepsy, thirty hours after the operation, when the wound was found healed, media clear, and result of operation excellent. This proves that the wound, even in persons of great age, will heal in twenty-four hours. In one (aged 65, Case 25) suppuration threatened for twenty-four hours, the patient ultimately recovering with a closed pupil, and fair chance of restored vision by a second iridectomy downwards. The remainder recovered without a bad symptom, and were discharged on the seventh day ; such as were free from remnants of capsule or amaurotic complications being restored to excellent vision. Some have been too recently operated upon to permit a fair record of the optical results at present. In two cases the lens was removed by the scoop, and in seven extracted by Mr. Teale's suction method. Brief reports of the whole of those cases operated upon by myself since November 1865 will be given in the concluding part of this paper. I have preferred to give *all* the cases, including the suction and scoop extractions ; and, where there can be no objection to this being done, have followed Dr. Mooren's example, and appended the names and addresses of the patients, so that there may be no room for doubt or cavil as to the results.—*Edinburgh Medical Journal*, *Feb.* 1868, *p.* 700.

68.—A NEW OPERATION FOR CATARACT.

By Thomas Windsor, Esq., Manchester.

[The changes in the treatment of cataract have been frequent in recent years; depression, discision, and extraction having in turn succeeded each other.]

What are the dangers of extraction and what the means by which they may be avoided? Even during the operation itself we are exposed to risk from unsteadiness of the patient, from violent contractions of the ocular muscles, and from an inaccurate manual performance on the part of the surgeon or his assistant. All these, so far as they are due to the behaviour of the patient, can be overcome by fixing the eye with forceps, and by the administration of chloroform. Immediately following the operation we have primary suppurative destruction (necrosis) of the cornea and primary iritis; the former is essentially due to the corneal incision, and the frequency of its occurrence rapidly increases as the latter includes a greater extent of the corneal circumference, though no doubt a diminished power of restoration has a considerable share in its production in the extremely old and in marastic patients. The latter (iritis) is caused by contusion of the iris, by the irritation of cortical remnants or of the retracting capsule. At a later period come secondary suppuration in the cornea and secondary iritis or irido-choroiditis. Secondary corneal suppuration is generally a consequence of a low form of iritis. Secondary iritis is caused by the extension of inflammation from the cornea or from the capsular epithelium. We may infer, if these views are correct, that to prevent primary suppuration, we should make the wound as small as is compatible with the passage of the lens, and of a form that will interfere as little as possible with the nutrition of the cornea, and that will keep the lips of the wound in apposition. To prevent primary iritis, we should remove both the lens and the unopened capsule.

I have endeavoured to fulfil these indications in the following manner:—The patient is kept throughout the operation fully under the influence of chloroform, so fully indeed, that he shows no sign of reaction when the conjunctiva of the eye which is not to be operated on is seized with forceps. A small* incision is then made at the corneo-sclerotic junction, the adjacent portion of iris is excised, the zonula is opened with a small hook close to the wound, the scoop is gently passed into the vitreous cavity just behind the lens, and the latter is slowly

* I am inclined to think that it matters little whether the wound is straight, as in von Grafe's modified linear extraction, or forms a small flap, *i.e.*, a flap of sufficient length, but little height. The wound must, however, be always of sufficient length, for example, always longer than Schuft's incision (3‴).

removed. The after-treatment may be much as after extractions; the eye may, however, be safely examined at a much earlier period, say 24 to 36 hours after the operation.

The scoop that I have used, and that seems to answer its purpose well, was made for me by Mr. Wood, instrument maker, Manchester.

My cases are yet too few, and most of them have been performed too recently, to allow me to attempt any complete account of the results: I can only say that I have been pleased and surprised with the rapidity of healing and the absence of any threatening symptoms.

A comparison of this operation with the others, to which I have already referred, will show that it promises all the advantages claimed for them, without their disadvantages; the only objections that can be raised are in respect to opening the hyaloid membrane and a possible loss of vitreous. As to the former, I believe the risk is extremely trifling; as to the latter, that no loss of any moment can occur, provided the patient is fully under the influence of chloroform. Its advantages appear to be, that the risk of the operation is very much lessened; that the cataract is completely removed by a single operation and no secondary operation is required; that the eye can be opened at an early period and be carefully examined without danger; that the convalescence is more rapid; that the patient may be sooner discharged with safety, and that the average vision is (probably) superior to that afforded by other methods.

It scarcely requires mention that the operator must expect some failures, especially in his earlier cases; the capsule is sometimes extremely delicate and will give way, but such accidents are likely to become less frequent as familiarity is gained with the method; on the other hand, I see no reason why such an operation should not be employed in some cases, at least, of soft cataract.

I have purposely omitted intra-ocular hemorrhage from my list of mishaps, because of its very rare occurrence: it would probably be still more uncommon after the proposed method than after flap-extraction, for the great and sudden relaxation of tension from loss of vitreous during the operation, and iridochoroidal congestion at a later period, which must so much promote it, would scarcely ever happen.

In conclusion, I may again repeat that *the object of this operation is to prevent primary corneal suppuration and iritis by the form of the wound and the removal of the lens in its unopened capsule.*—*Ophthalmic Review, April*, 1867, *p.* 251.

69.—ON LINEAR EXTRACTION OF THE LENS.

By C. Macnamara, Esq., Surgeon to the Calcutta Ophthalmic Hospital.

[The writer commences by stating that he has performed the operation of linear extraction on upwards of 350 patients in hospital since 1864, and that in his private practice he very seldom resorts to any other proceeding in cases of hard cataract.]

In a former paper I inculcated the practice of excising a portion of the iris before attempting to extract a hard cataract from the eye. This proceeding is generally followed: in fact, Mr. Bowman says (p. 368, No. 4, 1865, of the Royal Ophthalmic Reports), "The iridectomy which is a necessary part of it, is no disfigurement, nor does it in any important degree affect the optical result." Further experience has led me to modify the rules I before laid down on this subject, as will be seen from the above table; for of the whole number of hard cataracts operated on (forty-two), I have only excised a portion of the iris in five cases. The plan I now adopt is, after the section has been made, and the capsule lacerated with the point of the knife, to pass the scoop so far into the anterior chamber as to enable me to reach the outer margin of the pupil with its blunt extremity, and, gently retracting the instrument, I draw open the pupil with it to such an extent as to allow of my pressing the edge of the scoop against the outer margin of the lens, which immediately tilts over on its axis, and the scoop being thrust onwards, the cataract comes to lie in the concavity of the instrument, and may then be withdrawn from the eye. By following out this plan, the scoop is never inserted deeper into the eye than on a plane slightly posterior to that corresponding to the natural position of the iris; and thus the vitreous is left intact. Upon this manipulation the success of the operation often depends. The matter is more fully described in my published lectures on the diseases of the eye. I would insist, however, on the fact that the scoop must not be thrust behind the lens as it lies imbedded in the vitreous; if this practice be pursued, there is considerable danger of an escape of this fluid, and its consequences. In two instances only out of the above one hundred cases, was there any loss of vitreous, and both of these went to the bad. I have little doubt that a fluid state of the vitreous existed prior to the operation, and no precaution on my part, as far as the operation was concerned could have prevented its escape. Von Graefe (Ophthalmic Review, No. 9, p. 11) states that of 118 cases operated on by him, "there were nineteen cases in which anomalies occurred in the course of the operation; viz., for the most part from the prolapse of the vitreous." I am inclined to attribute the difference in the conclusions which Von

Graefe and I have arrived at on this point, to three causes: first because I am not in the habit of thrusting an instrument behind the lens till its attachments to the posterior capsule have been broken down, but I make the lens turn over on its own axis till it comes to rest in the hollow of the scoop; secondly Von Graefe does not administer chloroform so freely as I am in the habit of doing; and, lastly, I make my incision through the cornea considerably anterior to its line of junction with the sclerotic.

To the first of these causes I have already adverted, and the subject requires no further comment; with regard to the second, I may observe, that I never, under any circumstances, operate by linear extraction, unless my patient is *fully* under the influence of chloroform, and certainly the statistics above given bear me out in the propriety of this proceeding, for out of the whole number only seven patients vomited after the chloroform, and every one of these made excellent recoveries; consequently the fear of my patients vomiting after the administration of chloroform is not a complication which I dread much; and if the patient is prepared beforehand, it seldom occurs at all. My practice is to administer a purgative two days previous to the operation; on the day preceding the extraction I allow my patients to take very little food after 1 p.m., and then operate early the next morning, before they have swallowed either liquid or solid food. The chloroform should be administered with a free hand, so as to get the patient under its influence speedily, and thus save him from inhaling any large quantity of the anæsthetic; and should there be any disposition to vomiting, a bag containing ice should be laid on the pit of the stomach; but, as I before remarked, I have not that innate dread of the effects of vomiting after linear extraction which appears to cling to some surgeons, especially if care is taken immediately after the operation to apply a compress and bandage over the closed eyelids. I do not, therefore, consider that the possible accidents which may occur after the administration of chloroform should for an instant allow us to refuse to employ it, knowing as we do the certain benefits which the vast majority of our patients must derive from its use. Chloroform not only renders extraction of the lens a comparatively easy proceeding, and therefore within the reach of any practitioner who is accustomed to operate; but it also prevents that injurious straining of the eye during and immediately following the extraction of the cataract, which is so much to be dreaded in cases of this kind. With regard to the position and size of the opening which is to be made in the cornea for the operation of linear extraction, this is doubtless a matter of considerable importance; for unless the incision be sufficiently large to allow of the easy

exit of the lens, plus the thickness of the scoop, it will be necessary to use considerable force in removing a hard cataract, and consequently damaging the epithelium of the elastic lamina of the cornea and iris. The incision should extend over a section corresponding to one-third of the circumference of the plane of the cornea, through which the opening is to be made; and unless we can succeed in doing this before the knife is withdrawn from the anterior chamber, it will be necessary to enlarge the opening by cutting through the cornea at either extremity of the incision with a pair of curved scissors, before attempting to remove the cataract from the eye. The secret of making this opening in the cornea sufficiently large in the first instance, consists in having a short but broad-bladed triangular knife; and secondly, in thrusting this instrument, when once its point is through the cornea, steadily forwards into the anterior chamber, so that the incision as it is made is filled up with the blade of the instrument, and the aqueous is prevented from escaping. Should this fluid rush out of the eye before the section is completed, the cornea at once becomes flaccid, and it is impossible to make the necessary opening through it with the knife; but it must then be enlarged, as I have above described, with a pair of scissors.

With regard to the position of the incision in the cornea, as a general rule I never make it within an eighth of an inch from the circumference of the cornea. I believe it is a decided advantage to have the incision well forwards in the cornea; if it is made too near the sclerotic, there is much greater chance of prolapse of the iris and escape of vitreous; nor can I see any objection to having the section in the position mentioned, so long as it is well away from the central axis of the eye, and in a plane sufficiently large to admit of the passage of the lens through it. I cannot say it gives me much concern as to the direction of the edges of the wound; unless some accident should occur, they unite within forty-eight hours, whether made at an obtuse, acute, or any other angle to the surface of the cornea.

The scoop which I usually employ is precisely of the same shape as that originally used by Waldau; but in addition, its curved extremity is serrated. This scoop is also considerably larger, and a hole exists through its central portion, so that, in fact, the rim of the instrument and its curved extremity alone remain. The advantage of this is the lens fits more firmly into this open space than it does into the spoon-shaped scoop; and in withdrawing the cataract from the eye, we have merely its thickness from before backwards to deal with, and not, as in the ordinary scoop, with the antero-posterior axis of the lens, plus the thickness of the instrument, to draw through the

section in the cornea. A scoop of this kind is supplied by Weiss in all the cases of linear-extraction instruments which he sends out to this country.

It appears to me a very important point to remove any lenticular matter from the anterior chamber that may be left there after the extraction of the lens. We may have to introduce the scoop half a dozen times into the anterior chamber before we can withdraw all the lenticular matter from the eye ; and if small particles of it get lodged between the upper or lower portion of the cornea and the iris, a curved scoop must be employed, in order that we may dislodge them. I consider it absolutely necessary, however, that these particles of lenticular matter should be removed. Pulpy cortical substance does less harm in the anterior chamber, but the smaller the quantity allowed to remain there the quicker and more certain the patient's recovery will be.

One of the great advantages of linear extraction over the flap operation is the comparative immunity attending the former from cyclitis and suppuration of the cornea. Both of these accidents were of comparatively common occurrence among my flap-operation cases. There appears to be a very general belief among certain persons that the natives of Bengal are peculiarly advantageous subjects to operate on,—that, in fact, they rather like having their limbs or lenses removed; that wounds heal up in a marvellous way; and that we have none of the difficulties to contend against in our after-treatment which are so frequently experienced in hospital practice at home. A greater mistake than this could not possibly exist. One has only to turn to Fayrer's account of osteo-myelitis to perceive that the class of patients met with among the poor of Calcutta are about as unfavourable a set as we could conceive to operate on; and, consequently, those who attempt the flap-operation for the removal of the lens, will find suppuration of the cornea and cyclitis, running rapidly on to abscess of the eyeball, by no means an uncommon sequence of their best efforts; but out of the 100 cases I have operated on by linear extraction during the year, four eyes only have been lost, either by sloughing of the cornea or abscess of the globe; and in my private practice I have not had a single instance of this kind occurring after linear extraction. This fact alone is sufficient to stamp the operation as one having peculiar claims upon our consideration. I am in the habit of dropping an eight-grain solution of atropine into the eye immediately after the operation, and then applying a compress and bandage over it. On the following morning this is removed, and extract of belladonna, containing four grains of atropine to the half-ounce, is smeared over the closed lids. The compress may be discontinued, and a simple bandage employed.

On the following day the lids may be gently opened, and the solution of atropine dropped into the eye. If we then find the pupil is dilated, there will be no necessity for the further use of the atropine; but should its effects on the iris not have been fully established, we must continue to use it every six hours until the pupil is well dilated. When once this has taken place, there will be very little chance of any subsequent mischief going on in the eye; but unless the pupil is dilated, it is impossible to say how the case may terminate. Atropine, and such other remedies as may appear necessary, must be employed; but, as a general rule, the after-treatment of a case of linear extraction is, comparatively speaking, a very simple matter, as compared with one of flap extraction.—*Ophthalmic Review, April* 1867, *p.* 239.

70.—MODES OF EXAMINING THE OPTIC DISC WITH THE OPHTHALMOSCOPE.

By C. Bader, Esq., Ophthalmic Surgeon to Guy's Hospital.

I. *Examination of "the inverted image," or indirect ophthalmoscopic examination of the optic disc.*—Suppose we wish to examine the optic disc of a healthy right eye with blue iris, the pupil having been dilated by atropia, and the observer, the patient, and the light, being placed as is usual in ophthalmoscopic examinations :—

We take the opthalmoscope in the right hand, grasping its margin with the thumb and forefinger, and turn the reflecting surface towards the patient. We "throw" the light upon the patient's right eye, and approach the eye with the ophthalmoscope until the circular surface of light on the patient's face has nearly the size of the ophthalmoscope itself. We must take care to keep the circular surface of light continually on the patient's eye while raising the ophthalmoscope to our own right eye. When looking through the sight hole, we perceive the patient's pupil to appear brilliant red instead of black. If the pupil appears black, we either are not throwing the light upon the pupil, or are not looking through the sight hole of the ophthalmoscope.

When we have obtained the red reflection from the pupil, we next look for the optic disc, which is most quickly found (the ophthalmoscope being held by the thumb and forefinger) by holding up the little finger of the same hand, and directing the patient to look at the tip of it. The centre of the yellow spot of the patient's right eye is thus directed to the tip of that finger, and the optic disc comes to stand opposite the pupil.

A change in the colour of the reflection from the pupil, from

red to whitish-red or brilliant white, indicates that the optic disc is in view. The observer should persist in his endeavours until he has obtained the brilliant whitish reflection.

In persons with dark irides, the difference in the reflection from the pupil is still more striking, on account of the contrast of colour between the optic disc and the tunics.

Having thus obtained the reflection peculiar to the optic disc, we place the convex lens (holding it between the fore-finger and thumb of the left hand) before the eye under examination. The diffused image of the flame, which is "thrown into the eye" by the ophthalmoscope, serves to illuminate those parts of the interior of the eye which lie within the area of that image. The rays of light which are reflected, *e.g.* from the optic disc thus illuminated, have to pass through the lens held in front of the patient's eye, and an inverted defined image of the optic disc, &c., is formed in front of the lens, *i.e.* between it and the observer, and is viewed by him.

In this case we do not directly examine the optic disc, but only its inverted aërial image; hence we term this method "the examination of the inverted image" or "the indirect ophthalmoscopic examination.

The little finger of the hand which holds the lens is placed on the patient's cheek to steady the lens, while the middle or ring finger is kept disengaged, to be placed, if necessary, upon the margin of the upper lid so as to raise it, or gently to press upon the eyeball, to produce and watch the pulsation of the retinal arteries in the optic disc. The lens itself is held at from one to two inches from, and somewhat obliquely in front of, the patient's eye. If the lens be held vertically the images of the opthalmoscope, which appear upon the two surfaces of the lens at its centre, interfere with the view of the part beyond. Hence, the lens should be held obliquely so as to cause the two images to recede from each other. By approaching the lens to, or removing it from, the eye under examination, we soon succeed in obtaining a distinct view of the well-defined (aërial) image of the optic disc, &c.

This image can be enlarged by our placing behind the sight hole of the ophthalmoscope a convex lens of greater focal distance (one of from ten to twenty inches focal distance).

II. *Direct ophthalmoscopic examination of the optic disc.*—Another mode of examining the optic disc with the ophthalmoscope is the direct method, or the examination of the erect image of the optic disc.

Though by the indirect method we can at one glance overlook the entire optic disc, we do not obtain such a correct idea of its colour, outline, &c., nor of its relation to adjoining parts;

neither do we see it so highly magnified as by the direct method. This method should, therefore, always be adopted when we wish to see the finer details of the interior of the eye. It is accompanied by the inconvenience of having to approach the eye to within one or two inches, but a few seconds suffice to obtain a view of the optic disc, &c., which no other method of ophthalmoscopic examination gives with equal facility.

The optic disc of the emmetropic and of the hypermetropic eye may be examined with the ophthalmoscope alone: to see that of the highly myopic eye with equal distinctness a suitable concave lens has to be placed behind the sight hole of the ophthalmoscope.

Though the direct method may be adopted without dilating the pupil of the eye under examination by atropia, it is better to employ this agent.—*Guy's Hospital Reports*, 1868, *p.* 510.

71.—SUBCUTANEOUS INJECTION OF STRYCHNINE IN CASES OF AMAUROSIS.

By A. Samelson, Esq., Manchester.

[We take the following short extract from a letter to the Editor of the British Medical Journal.]

Within the last few years, there have been recorded three or four cases, sufficiently authenticated, it appears (by Fréynineau, Sämann, and others), of complete amaurosis, with entirely negative ophthalmoscopic evidence, in which the subcutaneous injection of strychnine has proved of sovereign advantage in restoring the sight of the patients, after varying intervals of total extinction, and after unavailing trials of other methods.—*British Medical Journal*, *Nov.* 23, 1867, *p.* 486.

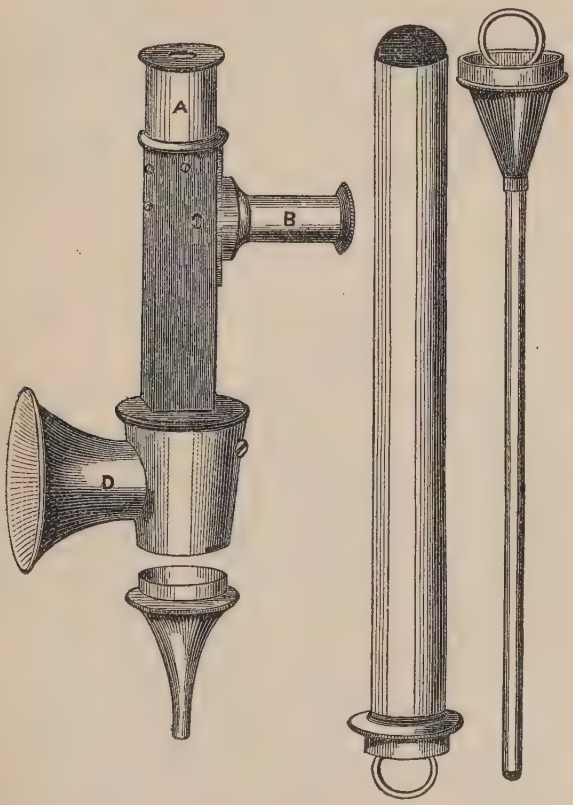
72.—A NEW INSTRUMENT FOR DEMONSTRATING THE MEMBRANA TYMPANI, OR OTHER DEEPLY SITUATED ORGANS.

By James Hinton, Esq., Aural Surgeon to Guy's Hospital.

I wish to lay before your readers a short description of two small instruments. The first was designed for the purpose of demonstrating the membrana tympani. I had long felt the difficulty of exhibiting that structure satisfactorily to a third person, inasmuch as, in any of the ordinary methods, it was necessary to remove my own eye from it before it could be brought within another person's vision, and without the aid of sight it is very difficult to maintain the speculum in its exact position. The instrument depicted below (Fig. 1), by the introduction of a

prism at C, enables two persons to see it perfectly at once, the eye being placed at A and at B. In other respects, it is essentially the same instrument that has been long in use in the hands of my friend Dr. Anderson, of Glasgow. The light is

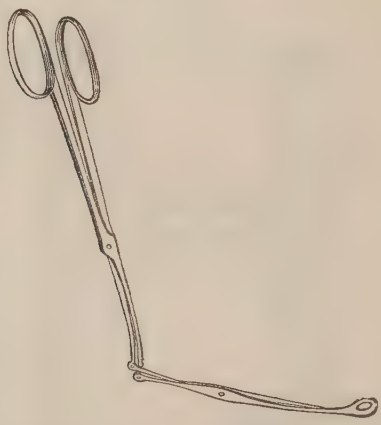
FIG. 1.



admitted at D. The refracted rays, of course, give a lateral inversion of the object, making the right hand appear the left; but this is of no practical importance. The instrument has

been found equally available for the demonstration of other internal organs—the rectum, urethra, &c.—and constitutes I believe, an exceedingly efficient and convenient "endoscope." The rectal and urethral tubes are also represented in the cut.

Fig. 2.



The second figure represents a forceps of a new construction by which great lightness and tenuity are combined with considerable strength and firmness of grasp. It is especially suitable for cases in which, as in the auditory meatus, it is desirable to impede vision as little as possible. Both instruments are made at a very moderate expense by Messrs. Mayer and Meltzer, of Great Portland-street.—*Medical Times and Gazette, Jan.* 28, 1868, *p.* 104.

73.—REMOVAL OF FOREIGN BODIES FROM THE EAR.

From a report on Hospital Practice by the Editor of the Medical Times and Gazette.

The method we are about to describe is so simple that probably it may have occurred to others, and it has indeed been recommended in print by Mr. Hutchinson some time ago. It has, however, not yet found its way into our best manual of Aural Surgery (Toynbee, by Hinton), which advises the disappointing plan of syringing, whilst others still recommend the dangerous use of forceps or scoop. Instead of trying either of these, let the surgeon take six inches of fine wire and double

into a loop ; then, having the patient placed on his side, pass the loop into the ear as far as it will go, and turn it a little gently. At the first or second withdrawal the foreign body will come out in the loop. The wire being flexible gives no pain, and cannot possibly do damage. It is almost certain to find its way round the foreign body, however deeply the latter may be placed, or however closely it may fit the cavity. Mr. Hutchinson asserts in its advocacy that it is very much easier to use, very much safer, and lastly that he has several times succeeded with it in cases where other means had utterly failed. The scoop he regards as especially likely to do mischief, since it involves pressure against the wall of the auditory canal. There is in the London Hospital Museum part of the temporal bone of a child who died in consequence of a small bean having been forced by the scoop through the membrana tympani into the inner ear.—*Medical Times and Gazette, March* 7, 1868, *p.* 261.

DISEASES OF THE SKIN.

74.—THE PATHOLOGY OF THE SKIN.

By Erasmus Wilson, Esq., F.R.S.

We cannot do better than study the pathological lesions of the integument side by side with the commonest and most universal of the diseases of the skin, namely, eczema. Eczema, as a word, simply means *eruption*, and it may well be said, that it is *the eruption* of the skin ; as the most common of the eruptions it is that which comes the most frequently under our observation, and is therefore the one to which we can most readily refer, the most easily study, and with which we can with the greatest facility compare other forms of cutaneous disease. The word eczema was given to this very common eruption by the Fathers of Medicine, namely, the Greeks ; but they also had another name for it, which expressed one of the most prominent of the characters or qualities of the disease,—namely, *psora*, and we must endeavour to pronounce the *p* as well as the *s* in this word to give it its proper effect. Now psora means *the itch*, because of its itchiness, because, in fact, of the necessity, which is induced by its itchiness, to rub and to scratch ; the word *psoein* means to rub, and *psoriaein*, to have the psora or itch. This is the language of Hippocrates, who dates back five hundred years before the Christian era ; and these words are as applicable to the disease in question at the present hour as they were in the time of our great predecessor. But let us pursue the matter a little further. "Familiar to our ear as household

words" are the terms eczema and psora, and so also is another word, psoriasis; but until now it may not have been known to you that eczema and psora have the same meaning, and are in fact synonymous; that eczema announces the *eruption* only, whereas psora represents the most marked and troublesome of its *symptoms*, its psoric or itchy tendency. By-and-by, we shall have to point out to you that eczema or psora presents two very marked and distinctive forms represented by the words *humidum* and *siccum*; namely, eczema humidum, eczema siccum, or psora humida and psora sicca. The moist form is distinguished by moisture or discharge, and the dry form by dryness and a consequent exfoliation of the cuticle in small scales; eczema siccum or psora sicca, is therefore squamous as well as being itchy, and in these two qualities combined we find the explanation of the word *psoriasis;* psoriasis, in fact, being the squamous form or stage of psora, of which it is an obvious derivative. It is important to remember this, because the word psoriasis has been generally misapplied since the time of Willan, and the evil has been increased of late years by the assignment of the term to the alphos of the Geeks, the lepra vulgaris of Willan. We also beg to remind you that the *a* in psoriasis should be pronounced long, namely, *psorīāsis.*

But we must say a few words more with regard to eczema before we commence to observe it more carefully. It may seem to you that we attach too much importance to the meaning of a word; but, gentlemen, we must ask you to accept in faith that which we now declare to you; we have not laboured at the study of cutaneous medicine for thirty years without having come to certain positive conclusions, and conclusions which we believe to be founded in truth. One of the first articles of cutaneous faith that we call upon you to subscribe to, if you would in future be ranked as our disciples, is, that eczema and psora are identical, and that psoriasis is a stage of psora. When the time comes, we shall be equally able to prove to you that that other disease which is now so commonly termed psoriasis by the foreign schools, is not psoriasis, but is really the lepra alphos of the Greeks, the lepra vulgaris of Willan. But, gentlemen, we have other masters besides the Greeks, and masters whom it is our delight to honour,—namely, those worthy successors of the Greeks, the Romans. Greatest among the Roman physicians was Celsus; and Celsus designates psora and eczema by the terms *scabies* and *impetigo;* scabies derived from *scabĕre*, to scratch, itself a derivative of *scaphein*, to dig as with the nails in scratching, is the analogue of psora; while impetigo, *ab impetu agens*, a breaking out with impetus, corresponds with eczema; and all the four terms apply to the same disease. At the present time, however, we dispose of these four terms

differently; eczema we retain as the type of the psoric affection; impetigo, we apply to a pustular eczema; scabies, we restrict to the eczematous eruption, excited by the acarus scabiei; psoriasis we give to the dry and squamous eczemata, especially when associated with thickening and cracking of the skin and much pruritis; and psora we discard, as having no special occasion for its use.

Gentlemen, we offer you no excuse for this digression from our subject; we have led you over classic ground, and through a territory in which disputed questions prevail, but we have endeavoured to guide you with the lantern of knowledge to the temple of the veiled goddess whom all true worshippers of science most venerate; and we conclude our episode by informing you that although in cutaneous medicine we are ungracious enough to have rejected the ancient term *psora*, yet that we retain it in our popular language in the simple and forcible expression *sore*.

Well, gentlemen, if we have a subject before us suffering from eczema, we shall inevitably find *redness* or hyperæmia more or less extensively distributed; we shall have *swelling* or thickening from infiltration of the affected skin; we shall possibly discover an elevation of temperature; and we shall pretty certainly be made aware of *pain*, commonly in the character of burning or scalding, itching, and probably tingling and pricking. But besides these morbid states, or lesions, the common signs of inflammation of the skin, we shall meet with minute elevations, which are termed *papulæ*, or pimples; or small elevations filled with a transparent albuminous fluid, namely *vesiculæ*, or vesicles; maybe with small vesicles filled with pus, vesico-pustules, or simply *pustules;* then we shall find *excoriations* exuding a viscous albuminous fluid, sometimes transparent and sometimes purulent; or the thickened skin may be fissured with *chaps;* or there may exist coverings of different kinds on the inflamed corium, sometimes assuming the character of *crusts* or *scabs*, and sometimes of small *scales* undergoing constant desquamation. Again, we may observe, in conjunction with these lesions, *scratches* denoting the operation of the nails for the relief of itching; and lastly, *maculæ* or stains left on the affected part after the healing of the skin.

Now all these *lesions* may be present at successive periods on the same person and in the same disease,—for example, eczema; and if we look at them a little more closely, we may see reason to divide them into two groups,—namely, such as are of earliest appearance, or *primary;* and such as are subsequent, or *secondary*. For instance, the redness, the papule, the vesicle, and the pustule are primary; while the swelling, the excoriation, the fissure or chap, the crusts, the scabs, the scales, the scratches, and the stains, are secondary. But besides these

lesions, which are all present in eczema, there are a few more which belong to other forms of disease, and therefore we shall assemble all that are usually recognised, under the two heads primary and secondary, and consider and describe them seriatim.

The *primary lesions* of the skin, or signs of a morbid state, are as follows :—

Rubor.	Pustula.	Tuberculum.
Papula.	Bulla.	Tuber.
Vesicula.	Squama.	Macula.

And the *secondary lesions* :—

Tumefaction.	Chap.	Desquamation.
Excoriation.	Ulcer.	Discoloration.
Scratch.	Crust.	Cicatrix.

Rubor or redness, is the state of colour of the skin produced by the abnormal distension of its vessels with blood. It may be simply *functional* and uncomplicated with any change in the vessels themselves, or it may be *structural* and depend upon a permanent state of enlargement, or hypertrophy of the vessels, or alteration in their coats. Functional redness may be present as a mere transient blush, or physiological hyperæmia, or it may represent many degrees of intensity of pathological hyperæmia and constitute an exanthema or an erythema. Structural redness, on the other hand, may be due to a permanent enlargement of the vessels of the skin, termed angeiectasia and vascular nævus ; or it may result from lesion of the coats of the vessels and the escape of the blood into the parenchymatous tissue, as in purpura, and also in contusion of the skin.

Pathological redness also presents the phenomena, that it may subside without alteration of nutrition of the skin, as in some of the slighter forms of erythema, or it may induce a suspension of nutrition of the epidermis, and in this way give rise to exfoliation and desquamation of the cuticle ; or it may be associated with exudation and hypernutrition of the cell-structure, causing thickening ; or, finally, it may become chronic and accompany the secondary series of morbid processes, and be in itself a secondary lesion. You remember, gentlemen, that it was upon redness that Willan founded his definition of *exanthema*, or *rash*,—namely, that it consists "of red patches on the skin, variously figured, in general confluent, and diffused irregularity over the body, leaving interstices of a natural colour." In this definition, however, it is clear that Willan had in his mind the rash accompanying the exanthematous fevers,—namely, rubeola, scarlatina, and variola.

If we incline to seek for examples of the several forms of pathological redness which we have enumerated, we shall find as

illustrations of hyperæmia subsiding without secondary changes, erythema, urticaria, and roseola ; suspended nutrition of epidermis resulting in exfoliation is seen in the exanthematic fevers ; and suspended nutrition of the epidermis with hypernutrition of the cell-tissue or exudation, in eczema ; eczema may also be taken as indicating a hyperæmia of a chronic character, and belonging to the group of secondary rather than of primary lesions.

Redness, therefore, is an important sign of cutaneous disease, and betokens either an excess of blood in the skin or an escape of blood from the vessels. The excess of blood may be physiological or pathological, and in the latter case it may be temporary, in consequence of resulting from undue action, or it may be permanent, from an alteration of structure in the part. Again, it will excite in our minds a varying degree of interest, according as it may be general or partial, of larger or smaller extent, of irregular or circumscribed figure, or as standing alone, or associated with other pathological signs or lesions, whether primary in their nature or secondary.

The papula or pimple is an elevation of the cutis, for the most part conical, but sometimes semiglobular or flattened, minute in size, of a variable tint of redness, generally accompanied with itching, and commonly succeeded by a thin scale. We will ask you to bear in mind the definition of papula given by Willan,—"a very small acuminated elevation, with an inflamed base, very seldom containing a fluid or suppurating, and commonly terminating in scurf." If we seek further into the nature of a papula, we shall find that it has its seat at the aperture of one of the follicles, of the so-called pores, and that it is produced by hyperæmia of the vascular plexus of the follicle and exudation into the intervascular parenchyma. The degree of redness of the papula will bear relation to the extent of hyperæmia, and its bulk to the amount of exudation. You must satisfy yourselves, at the first opportunity, of the exact seat of the papula, and the knowledge you will thereby acquire will help you very materially in comprehending the pathology of the skin. With careful scrutiny you will discover the aperture of the follicle at the extreme summit of the papula, and you will find the summit to be transparent, from the presence of a conical plug of epidermis which normally occupies the dermal entrance of the follicle. Rayer and others pricked the summit of the papula under the expectation of finding an explanation of this transparency in the presence of fluid ; but they were naturally disappointed, having mistaken the cause of the appearance.

Besides its conical or semiglobular or flattened figure, we have to note, in connexion with papula, the disturbance of innerva-

tion to which it gives rise and the consequent pruritus. The papula is remarkable for its pruritic tendencies, but we are unable to say with any certainty whether the itching is due to pressure on the filaments of the terminal nervous plexus by the hypertrophied cells of the parenchyma, or to absorption, by the nerves themselves, of the exuded fluid of the vessels, or of the excreted fluid of the cells. In a few instances the papula subsides to the normal level of the skin without desquamation; more frequently the morbid action of the tissues determines an arrest of nutrition and the consequent exfoliation of the cuticle covering the summit of the cone, while, in not a few instances, the itching provokes scratching, by which the summit of the papula is torn off, and a scab is produced, either through the discharge resulting from excretion by the over-distended cells, or from the escape of a minute drop of blood, the exudation in both instances desiccating by evaporation on the summit of the papule. The duration of an individual papule ranges from a few hours to one or two or more days.

As redness is the type of the exanthemata and the erythemata, so prominence is the type of the *papulæ* or papular affections, and especially of lichen. We need not stop here to inquire by what perversion of signification the term lichen, meaning literally a tree moss, has become symbolized by a papula of the cortex of man; but we shall do well to accept it as a fact. By lichen we understand a papular eruption, and we have other examples of a papular eruption in strophulus and prurigo. If we go back to the illustration of cutaneous disease which we selected as a standard of comparison—namely eczema—we shall find papulæ to be a constant element of that eruption, and sometimes a most conspicuous symptom, as in the variety which we term eczema papulosum and also eczema lichenodes; and the papule of eczema is true to the characteristics of its family stock, the papules are minute, they are conical, hard to the touch, and extremely pruritic. Strophulus is a papulous eruption occurring in infants, and the pimples are larger than the typical papula; they are frequently rounded at the summit instead of being conical, and they have an amount of redness and exudation around their base greater than is met with in the typical lichen. The papulæ of lichen planus are remarkable for their flattened and depressed summit, while those of prurigo are remarkable for the extreme degree of pruritus by which they are accompanied; they are often not perceptible until the pruritus has excited scratching, and the scratching has, as it were, called them into being, and they are especially distinguished by the small black scab resulting from exuded blood that crowns their summit after they have been violently torn with the nails. There is another papule which is remarkable for a tingling itching, which is larger

than the papule of simple lichen, which has the pale hue of the tubercles of urticaria after it has been scratched, and which, like the papule of prurigo, is followed by a small black scab : this is an eruption of children, and is termed lichen urticatus. Then we find in our catalogue of papulæ, the minute papules of rubeola and scarlatina ; the papules of variola, hard and gritty at first, and afterwards undergoing development into vesicles and pustules ; the papules of syphilosis, distinguished by their dull-red colour and the absence of pruritus ; the papules of gutta rosacea, and the large follicular papules of acne and sycosis, together with other forms of papulæ dependent on accumulations in the follicles of cellular exuviæ or sebaceous substance ; on hypertrophy of connective tissue, as in the instance of achrochordon, or on hypertrophy of papillæ, as in verruca.

If, therefore, we proceed to sum up the special characters of a papule, we shall find them comprehended in a consideration of its size, figure, colour, seat, symptoms, and decline. Its *size* ranges between a line and two lines, or, may be, three—that is, a quarter of an inch : the latter would be esteemed a very large papule, verging on a tubercle, and our measurements of the papulæ of cutis anserina give a diameter of half to three-quarters of a line ; while in height it averages about a line. Its *figure* is, for the most part, conical, sometimes globose, and sometimes flattened or depressed in the centre, as in lichen planus. Its *colour* varies from white to red, and from red to its deeper tints, reaching even to the confines of purple and livid ; the white colour is seen in lichen urticatus, the bright-red in lichen simplex, and its purple and livid hues in lichen lividus. The *seat* of the papule is very obviously the aperture of the follicle, and from the follicular plexus it derives its colour and its prominence. Its most characteristic symptom is pruritus, and its decline is accompanied with the separation of the circle of cuticle which originally formed the covering of its summit.

The papule, which we have just been describing, might be called the papula of Willan, or the hyperæmic papule ; for we need not remind you that the term pimple is commonly employed in a more universal sense. According to Hebra, a papula is "a solid projection above the surface," consequent on "any morbid change in the skin," and presenting a size varying between that of "a millet-seed and a lentil, and containing no fluid." In reference to *size*, he names the papulæ *miliares*, *miliformes*, and *lenticulares;* and in point of *figure*, he treats of them as being acutæ, planæ, conicæ, and globosæ. These words of Hebra reminds us of the use which we sometimes make of seeds and coins as a comparison of size. Thus, there are the millet-seed ; the mustard-seed, black and white ; the hemp-seed ;

the lentil; the pea; the bean; and several kinds of nuts, of eggs, and of fruit; besides the coins in common use, the smaller and the larger silver pieces, and the copper pieces. But who amongst us can form anything more than an approximate guess of the size of an object from the use of such terms? who has determined by experiment that the size of the black mustard-seed is about a half a line in diameter; the millet seed, one line; the white mustard-seed a line and a half; the hemp-seed, two lines; the split pea about three lines, that is, a quarter of an inch; the lentil, more than a quarter of an inch; the four-penny piece, more than half an inch; the sixpenny piece, three quarters of an inch; the shilling piece, one inch; and so on? We know how easily we can be deceived in the size of objects; and, therefore we advise you, very seriously, in your essay to distinguish size, to have recourse to fractions of an inch, be they lines or eighths, in preference to the before-mentioned objects. When Hebra speaks of papulæ miliares, we may understand papules having the diameter of a line; but when he mentions papulæ lenticulares, we hesitate to accept the comparison, because the lentil measures more than a quarter of an inch in breadth, and prominences having such a bulk we should at once designate as tubercles.

Hebra also reminds us that the range of signification of the word papule, or pimple, is much more extensive than our definition is calculated to admit. For example, besides the common papula produced by exudation, whether in the follicles or in the papillæ, there are the prominences resulting from muscular spasm, constituting cutis anserina; accumulations of cellular exuviæ in the follicles; accumulations of sebaceous matter in the follicles; abnormal structure of the sebaceous gland, by Hebra termed "degeneration," while we should call it arrest of development; hypertrophy of papillæ; and hyperplasia, or new formation of papillæ; to which he further adds, hemorrhage into the rete mucosum, and illustrates this state by reference to Willan's lichen lividus,—an evident mistake, since the lichen lividus of Willan is nothing more than purple and livid papulæ. It is well, however, that we should remember that the word pimple has a general meaning in addition to its specific and technical signification.

The Vesicula is a prominence of the epidermis containing an aqueous fluid, of minute size and variable shape, being sometimes conical in figure, more frequently semiglobular, and sometimes flattened or depressed. Willan defines it as "a small orbicular elevation of the cuticle, containing lymph, which is sometimes clear and colourless, but often opaque, and whitish or pearl-coloured; and succeeded either by scurf or by a laminated scab." The average range of size of a vesicle is half a line to

two lines, and its height somewhat less than the breadth of its base. A vesicle three lines in diameter, that is, of the bulk of a small pea, must be regarded as of large size; and if it be larger, we should term it *phlyctæna* and *phlyctis*, or, following Latin phraseology, *bullula* and *bulla*. The vesicles of the smallest kind are those of eczema; namely, about half a line in diameter; those of miliaria are as large, and generally larger than a millet-seed, the diameter of the latter being one line; then above these we have the large vesicles, the almost phlyctænæ of herpes, as large as moderately-sized peas, measuring two and three lines in diameter, and leading upwards to the bullæ of pemphigus.

The pathological seat of the vesicle is the same as that of the punctum of hyperæmia or of the prominence of papula,—namely, the aperture of a follicle; and when a vesicle increases in bulk it takes in one or more neighbouring pores. Its shape is governed by the degree of resistance afforded by the cuticle, and in some measure by the force of the exudation; in the neighbourhood of the hair-follicles, the vesicle is semiglobular, sometimes irregular in outline, or angular, in accordance with the figure of the areæ of the lines of motion; sometimes solitary, and sometimes clustered around the opening of the follicle like a row of beads. On the finer skin between the fingers, where there are no hairs and only perspiratory pores, and where the cuticle is thin and moist, the vesicles are conical in figure; and under the same conditions, on the trunk of the body, and where the hyperæmic base is greater than between the fingers, the vesicles are semiglobular, as in miliaria. These same conditions, and especially the thinness of the cuticle, are the occasion of the brilliant transparency of the conical vesicles developed between the fingers in scabies, and, *par excellence*, of the hemispherical vesicles of miliaria. On the other hand, wherever the cuticle is thick and dense, as on the palm of the hands and the palmar surface of the fingers, the minute effusions of lymph fail in the power of lifting the epidermis into vesicles, but may be seen through the cuticle in the form of globular cavities, and where they are very numerous, they are apt to lift up the entire epidermis of the part; for example, the whole of the palmar surface of an internodial portion of a finger, or a considerable extent of the palm of the hand. At other times, in consequence of the development of vesicles in great numbers, and often in clusters, they communicate with each other, and form beneath the cuticle multilocular spaces, sometimes of considerable extent; vesicles, therefore, may be dispersed or scattered over the skin, when they are termed *discrete;* or, they may be congregated in great numbers, so as to touch each other by their bases, when they are said to be *coherent;* or, they may be still

more closely packed and communicate by their cavities, when they are termed *confluent*. An eruption of eczema vesiculosum generally presents all these forms at the same time; in miliaria the vesicles are nearly always discrete; while in herpes, when the clusters are full, there is always some confluence of the matured vesicles.

A vesicle always makes its first appearance in the immediate circumference of a pore; it may be on one or other side of the pore, or it may form part of a row which surrounds the pore; or if, on the one hand, the pore be small—for example, that of a sudoriparous duct,—or the vesicle itself be moderately large, the vesicle may cover the entire circumference of the pore. In the latter case the aperture of the pore may be seen on the summit of the vesicle; or it may be tilted to one or the other side; and sometimes, by its connection with the sheath of the follicle, it is held down in such a manner as to flatten the summit of the vesicle, or to give the summit an indented, or, as it is commonly termed, an umbilicated appearance.

It must be mentioned, also, that vesicles present some differences in their contents, having reference to their age: at their first appearance the fluid which they contain is limpid and transparent; in the course of a day it is lactescent and opaque, and at a later period it becomes yellowish. Pathologically it is an albuminous lymph, more or less tenacious and viscous, and its opalescence is due to the occurrence of changes in its composition which lead on to the production of pus, and convert the vesicle into a pustule. We have examples of these changes in eczema, which convert an eczema vesiculosum into an eczema pustulosum; we see it also in herpes, and still more strikingly in vaccinia and variola. In both the latter affections the first developed of the primary cutaneous lesions is redness; then follows a papula, hard and gritty to the touch; then a vesicula; and finally a pustule. In miliaria, on the other hand, the change is limited to opalescence, and scarcely ever runs on to the purulent stage. It must also not be forgotten that a vesicular eruption may be subjected to pressure or friction, and that such violence may result in disruption of the vessels of the derma, and the effusion of blood into the vesicles; in such a case the contained fluid may be pinkish or reddish, or even purplish or black in colour; but the nature of the vesicle remains the same; and this occurrence is only to be regarded in the light of an accident.

Older pathologists were wont to look upon the production of a vesicle as the consequence of a passive transudation of the fluid part of the blood from its vessels; but the distinguished Virchow and his school treat of it as a physiological operation of the cells. According to the former, the serous fluid was

poured out upon the surface of the corium, and so lifted the epidermis in totality from off its basementary bed; but, at the present day, and in conformity with the cell-theory, we look upon the transuded fluid as a product of the rete mucosum, absorbed from the tissues of the corium by the cells of the rete, and exuded from the latter at its surface, so as to break up the connexion of the mucous and horny layer of the epidermis, and lift up the latter in the form of a dome. It is also to the vital operations of the rete mucosum that we must look for the explanation of the opalescence of the transparent lymph that first makes its appearance, and for the development of pus-globules and the conversion of the lymph into pus, as in some instances occurs.

In speaking of redness as a lesion of the skin, we had occasion to remark that hyperæmia was sometimes followed by exfoliation of the cuticle; the termination of papula is, in general, a thin scale corresponding in size with its inflamed base; but vesicula, as it combines with the hyperæmia of papula the production, besides, of a morbid secretion, gives rise to a more decided and thicker scale, and, in general, a *crust*. When a sudden outbreak of eczema vesiculosum, such as the eczema solare of Willan, quickly subsides, the contents of the vesicles are dispersed, partly by absorption and partly by evaporation, and the separated cuticle dries up into a thin scale, and is cast off in due season, of a size and thickness scarcely greater than that of papula, excepting in the instance of a coherent or confluent eruption, when the desquamation would be more extensive. Miliaria terminates in this way by a thin and almost inappreciable scale. But when the vesicle lasts longer, and runs through its opalescent to its pustular stage, it is apt, by the desiccation of itself and of its contents, to form a covering of considerable thickness, which is no longer a scale, but a scab or a crust. Such are the thick and hard scabs of herpes, firmly and deeply embedded in the skin, at first amber-coloured, then brown, and sometimes black; remaining apparent for a considerable time, and leaving cicatrized pits when they fall off. Of another kind is the crust formed by eczema, when it assumes a chronic character, and pours out a morbid secretion. In this instance it is not the vesicle which forms the crust, but the secretion exuded from the diseased surface. This secretion is partly serous and partly purulent, and, not unfrequently, it is discoloured with sanguineous effusions: hence the crust varies in consistence, in thickness, and in colour; it may be dense or friable; it may be thick and porous, or laminated; and it may be greyish, or brownish, or greenish, or yellow, reddish, or almost black. Crusts of this kind are met with on the face and scalp, in cases of ichorous and pustular eczema, and especially in eczema

infantile, in that frightful-looking affection termed *crusta lactea*. But *crustæ* will form a theme for future consideration, as being one of the secondary lesions, and we allude to them here, partly in consequence of being a mode of termination of vesiculæ, and partly because we desire thus early to call your attention to the three words, scale, scab, and crust. *Scale* is the thinnest of the three, and may be the separated cuticle alone, or the cuticle in conjunction with a thin varnish of desiccated albuminous secretion; *scab* is the desiccated vesicle, together with its contents, and sometimes includes a portion of the deeper tissues involved in destruction, as in herpes; while *crust*, however thick and hard, is superficial, and the consequence of the desiccation of morbid secretions. If you look to your dictionary, you will find that a *scale* is a thin lamina, anything that is exfoliated or desquamated; a *scab* is an incrustation formed over a sore by dried matter; and a *crust* is a collection of matter into a hard body.—*Journal of Cutaneous Medicine, July* 1867, *p.* 124.

75.—ON THE TREATMENT OF LUPUS.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Diseases of the Skin.

The internal remedies for lupus may, I think, be very safely divided into three classes,—those which possess a certain amount of value, those of very doubtful value, and those of no value at all.

The only remedies which I have observed to possess an undoubted control over lupus, and which will, I believe, in every case effect a certain amount of good, and in a certain proportion of cases bring about a cure, are arsenic and calomel in lupus of the face, and iodide of potassium in lupus of the limbs. Of course, there may be others, some known, some to be yet found out; I am limiting myself here strictly to facts which I have been able to verify.

Among the remedies of doubtful value I would put all the salts of barium, antimony, soda, mercury, iodine, and potass, with the exception of those in the first class; all combinations of salts of these, such as iodide of mercury, Donovan's solution, &c.; all the oils, such as cod-liver oil; and all vegetable preparations whatsoever. In ranking these remedies as doubtful, I do not in any way seek to invalidate the conclusions formed by some writers as to their power over lupus; our knowledge of them may be as yet in its infancy; for instance, with respect to the chloride of barium, which Dr. Frazer, a most careful observer, has seen cure lupus of the limbs. What I mean is this, were any given number of cases of lupus put before a

surgeon, he would not be able to say that *with these remedies only* he could rely upon curing a certain proportion of them, or perhaps a single case. The disease is often benefited for a certain time and to a certain extent by some of them,—for instance, iodine and iodide of potassium in lupus of the face; but in all the cases I have seen the amendment was very slight, and soon came to a standstill. M. Devergie treated twenty-six cases in this way for three months, and not one of them was cured. Cod-liver oil too produces often a degree of improvement, but it is equally uncertain. I would always give it as an auxiliary, but would never rely upon it to the exclusion of curative means.

Among remedies of no value, I think, we may safely rank steel, quinine, mineral acids, bitters, sedatives, alteratives, and sudorifics, change of air, sea-bathing, baths of every kind, and possibly many of the preparations naturally falling into the second class. This may seem going too far; I can only say that I see no other conclusion to be drawn, as I never noticed any beneficial change in the lupus produced by any one of these remedies. For instance, I have notes of several cases in which iron was prescribed sometimes by myself, sometimes by others. Among the preparations used were the tincture of the hydrochlorate, the magnetic oxide, the iodide, the sulphate, and the freshly prepared carbonate, made by mixing solution of the sulphate with solution of carbonate of potass, Griffith's mixture, &c. In no one instance did any of those preparations exert the slightest appreciable influence over the disease. Sometimes the health underwent a certain degree of improvement, but generally there is not much to improve. In the same way when mineral acids, such as the nitric or nitro-muriatic, were given, the appetite became more keen, and possibly, were the whole community treated in this way, a certain number out of every thousand would exhibit the same results; indeed it would be folly to dispute their power in promoting *the health*, their value as *auxiliaries;* but as to the influence of these remedies *over the disease*, they are, for anything seen to the contrary, as capable of producing as of alleviating it.

Change of air and sea-bathing are often recommended in this complaint, as they would be recommended for the results of over-work, anxiety, and confined air; for beginning mischief in the brain, or the lingering mischief of bronchitis. Yet it has nothing in common with any of them. It pursues its relentless course with equal severity on open plains and crowded alleys, among the hills of Cumberland and in the bracing air of the east coast, in the dry, bright climate of France, and the perpetual drizzle of Ireland or West Scotland, and often rages with greater severity in the place the patient is sent to than in that which he

has left. Under such circumstances only a very sanguine mind could look for benefit. Benefit may, it is true, follow change of air, but so may a very decided relapse; and the circumstances which determine either have not yet been so far elucidated as to enable us to say more than that it is a hazardous step.

With very few exceptions the external applications recommended in lupus are useless and impracticable. For instance, soothing or cooling, antiphlogistic or alterative dressings, ointments and lotions, leeches, &c., are, I believe, perfectly useless. Were they continued for a lifetime, it is very doubtful if they would ever check the march of the ulceration. Again, all remedies strong enough to give severe pain, such as chloride of zinc, Vienna paste, Canquoin paste, are and ever must be inapplicable, for the simple reason that, however valuable they may be, a very large number of patients will never suffer them to be used; others having once submitted will never allow a second trial to be made, and a third class will only consent when it is too late prevent irremediable deformity. As to whether they effect a cure or not when they have fair play, that is beside the question; the difficulty is getting them fair play; and whatever may be said in their favour, I apprehend their use will always be restricted to a small proportion of cases, principally among patients not very susceptible of pain, or unusually resolute in bearing it, and not easily deterred by failure or relapse,—for both will occur; or where one small solitary part is invaded by the disease.

For the purposes of treatment, all cases of lupus may, I think, be advantageously divided into two great groups; one embracing lupus of the limbs only; the other lupus of the face and head, complicated or not by affection of the limbs. The arrangement is, I admit, extremely unscientific; but there is reason to think it will prove effectual in examining the action of medicines, the varying power of which over different structures, and even the same structure in different parts, has not yet attracted the attention which, I think, it might fairly claim. Some years ago I endeavoured to call attention to this subject, and attempted, by a series of proofs, to show that medicines which are invaluable in a simple inflammation of one structure are powerless against the same affection in another; thus, for instance, opium acts most beneficially in peritonitis, and inflammation of the subcutaneous cellular tissue is often promptly checked by the free use of large doses of tartar emetic, yet this salt seems to exert no influence over peritonitis, while inflammation of the cellular tissue is only controlled by it in so far as the pain is relieved. Again, the very same inflammation in different mucous membranes, as is seen in ophthalmia and urethritis, may run a widely different course, and be very differently affected by the very same remedial measures. It would lead the reader too far

out of his way, were I to detail all the statements upon which this view is based. It must suffice to point out the general principle and to draw attention as far as possible to it, because it offers a nucleus for future observations, around which other facts may accrete, as crystals form round a nucleus.

So far as I have been able to observe, the most efficient remedy against lupus of the limbs is iodide of potassium. I am not speaking here of tertiary ulceration, but of that form which is often confounded with it—obscure lupoid ulceration, unconnected with syphilis or lupus of the face. For the most part, it only requires to be given in very moderate doses, and the necessity which exists in syphilitic cases for prescribing large quantities and steadily raising the amount given, does not obtain here. In general, a drachm weekly is quite enough at first, and it is seldom necessary to go beyond twice this amount. When it cures the disease, I believe it always acts soon, and the action goes on till the part is healed. When the improvement comes to a standstill, I am disposed to think that augmenting the dose will have no effect.

As to the mode of giving it, that may be safely left to the discretion of the surgeon. Perhaps one of the bitter infusions will answer as well as anything. One precaution should never be omitted,—that of getting the salt from a source where we can rely upon having it pure. It is not, perhaps, so much adulterated now as it used to be; indeed, I have been told, upon very good authority, that the adulteration rarely exceeds ten per cent., though, judging from the great difference in the action of prescriptions made up at different places, I should have considered this statement below the mark. Still even an admixture of ten per cent. may make a very material difference.

Should the bowels be confined, a mild pill may be given; in fact, in this as in any other complaint, all complications should, as far as possible, be swept away, care being taken at the same time not to interfere with the action of the iodide; for instance, in giving aperients, the alterative action of mercury should be guarded against. But I should think it was quite unnecessary to enter into details upon such a point; to keep on repeating that when gout is present colchicum combined with salines may be prescribed, and that iron is called for in anæmia; that impaired digestion and painful menstruation must be relieved; that we must attend to the general health and improve the secretions;—every surgeon knows, or ought to know, all this.

When we have to deal with lupus of the head or face, I believe the first remedy to be given is arsenic. Unless some disorder of the health, such as loss of appetite, great weakness, or anæmia is present, all preparatory treatment is, I think,

wasted; but when the tongue is coated and marked by the teeth, the breath foul, the bowels confined, and the appetite bad, a saline may be given for a few days, followed by nitric acid in bitters. The only question worth considering is the mode of giving the mineral and the dose suited to the case in hand. As to the mode of giving it, I believe that most of those who have tried De Valangin's solution (liquor acidi arseniosi hydrochloridi), consider it one of the best preparations ever invented, possibly superior even to the old Fowler's solution. Twelve minims may be given three times a day, with the food or directly after. It should be taken regularly, and the dose should always be measured out in a glass measure. Should this dose produce no disturbance of the stomach or bowels, no nausea or headache, no languor, or irritation of the skin, it may be brought gradually up to about sixty, or in a very few cases seventy minims daily. The former quantity is generally quite enough, as the medicine has to be continued for a long time. Perhaps simple water is as good a vehicle as could be devised; but so long as the efficacy of the arsenic is not interfered with by the addition of alkalies, mercury, or iodine,—so long as what is given with it can be given for a long time without any injury to the health—it may be prescribed in any form; but the simpler the better.

But though it will perhaps always effect a certain degree of improvement, it can scarcely be relied upon in a certain percentage of cases for producing a radical cure of lupus. It has been vaunted as a specific for the eating variety. I can only say that I cannot confirm the view. So long as the improvement seems to progress, however slowly, it would not be wise to interfere with it; it will often cure single-handed, and we thus sweep away one more complication from treatment; but so soon as the amendment comes to a standstill, so soon as ever a sufficient time has elapsed to form a valid reason for believing that it is really doing no good, and still more, as soon as ever there is a threatening of relapse, the use of mercury should I think, at once be resorted to. I know of no remedy which so quickly checks the spread of lupus as calomel. With it I have repeatedly arrested a sudden outbreak in a patient actually under the influence of arsenic; and what is more, the same result has occurred in the hands of others, who have thus been led to the same conclusion as myself. The fact has been so often noticed by the patients themselves, that so far from dreading the continued use of mercury, they are often more anxious to take it than I am to prescribe it, although I have never yet seen any harm ensue from the proper employment of this remedy.

It may be given in the form of a pill, mixed with any

substance likely to lessen the griping and pinching pain which it may cause in a few cases. A grain or two will, as a rule, be enough to begin upon; but as in the majority of cases this dose speedily loses its effect, it will in such cases be necessary to raise it at the end of a week or two. On the average this quantity does not require to be often increased, and then not largely. From two to three or four grains may in general be considered the outside limit, but no certain rule can be laid down. The only sure test is its purgative action: two, three, or four loose stools should follow each dose, and when this does not ensue, a saline aperient, such as the effervescing citrate of magnesia, a vegetable aperient like senna, or what is perhaps the best, a compound black draught, may be given the morning after; and a compound can be used, because the object here is not to investigate the action of either senna or magnesia, but to assist the action of the calomel, which is being investigated. With proper care there should be no action on the gums; but if any set in, mercury should be given up, and only the draught prescribed till this symptom has passed off. To prevent all needless repetition, it may be said here that these directions hold strictly good with respect to giving calomel for lupus of the limbs, except that it is generally less often called for and for a shorter time.

When there is a large open surface, the liquor plumbi may be used. The fluid should first of all be warmed, by placing it over hot water, and so soon as it begins to smoke it should be painted several times over the surface. What is left had better be thrown away. It ought to be applied at least once daily, all crusts and exudations being previously removed, and if the patient thinks any relief is obtained from its more frequent use, then let it be used more frequently. I am indebted to some of my correspondents—Dr. Hinds, of Birmingham, and others—for information about its value, and I am disposed from several trials I have made, to think very favourably of the remedy. Mr. Erasmus Wilson recommends it in lupus erythematosus and in scrofuloderma.

The acid nitrate of mercury is a very valuable remedy. When its use is restricted to small surfaces, to tubercles and to patches in which the morbid action has been a good deal subdued, or which are healing, but so slowly as to justify a resort to any means which will hasten the process, it is often of great service. With proper care, the pain attending its employment is so trifling as to constitute no valid objection. It should at first be brushed very lightly over the part, and a basin of water should be at hand to bathe the surface with immediately; but familiarity soon breeds contempt here, and the patients generally cease to avail themselves of the chance of washing the acid away. The

application should be repeated daily, both because the crust which is thus formed constitutes an almost impregnable barrier against the impact of the air, but also because the acid acts more painlessly than when only occasionally laid on. How it acts I do not profess to understand,—possibly by coagulating the albumen in the epidermis or the blood. Subsequent dressing (except in the eating form, when water dressing may be used) is, I think, superfluous; the principal thing is to protect the surface from the air, especially cold raw east winds, under the malignant agency of which lupus will sometimes relapse in a few hours as much as has been gained in a week; indeed as far as possible, I should say the patient ought to be confined to the house. Dr. Gilchrist, of Torquay, gave me the particulars of a case in which a lupoid ulceration of the nose was cured apparently solely by excluding the air.

For cases intermediate in severity between those which are suited for the liquor plumbi and those which require the acid nitrate of mercury, I should say the nitrate of silver is one of the best. At the beginning I would suggest a very sparing use of it in solution, not more than ten grains to an ounce. This quantity, however, may be *rapidly increased*, and after a few applications, the solid nitrate may be resorted to. Dr. Purdon, in a valuable communication which he has favoured me with, states that he gives the patient chloroform and then bores the nitrate deep down below the surface of the ulcer. His treatment seems to have been very successful. It is also a favourite remedy with Hebra, Dr. Alexander Anderson, and many others.

To my thinking, a lupoid surface should only be washed with very hot water, and when soap is used it should be of the most unirritating kind that can be procured. The diet ought to be good, and fat ought to enter largely into it. Where fresh fat meat cannot be borne, I would suggest the daily use of mild bacon or ham for breakfast. Too much meat is as injurious as too little; about half a pound a day for an adult has always seemed to me enough. Too much beef tea, jelly, or strong soup, like over-feeding, seems to me to overtask the digestion without aiding the nutrition, and this mistake is more than any other to be particularly guarded against when the patient is taking tonics. From any ale—except pale ale—I have never seen any good come; but a free allowance of beer, or, where it can be procured, good port wine, is invaluable. By far the best stimulant, however, is, I think, pure old rum given in milk, which possibly supplies something deficient in the nutrition.

The results of this treatment have been even more satisfactory than when I brought forward the subject last year. So far as

all the inquires I could make have yielded any response, it is to the effect that only two cases have relapsed, and in both cases the patients did their very best, not only to bring about a relapse, but to aggravate it to the very utmost; whereas in three of the cases, then to some extent doubtful, a perfect cure has, according, at least, to the reports of the patients themselves, taken place; and one which seemed hopeless had, when last seen, greatly improved. Other cases have gone on equally satisfactorily, which want of space will not allow me to dwell further upon.—*Journal of Cutaneous Medicine, July* 1867, *p.* 149.

76.—ON ACNE AND ITS TREATMEMT.

By Dr. Marris Wilson.

The Medical Times of 1859 contains a notice of Dr. Rodet, who recommends the following ointment as serviceable in all the forms of acne. It is composed of — Washed lard, 50 parts; sublimed sulphur, tannin, of each 4 parts; laurel-water, 5 parts. The proportions of sulphur and tannin are to be increased, according to circumstances, to 6 or 8 parts. The author, however, throws a doubt over the special fitness of this treatment, when in a succeeding paragraph, the same ointment is advised for sycosis, an affection of the hair system, so different in cause and situation from the disease of the sebiparous system, and so widely separated by modern classification. For sycosis the ointment is prescribed after the inflammation has been attacked and the crusts detached.

In the same journal is presented the treatment advised by M. Ferrat, extracted from the Bulletin de Thérapeutique, 1859, which very closely resembles that reported in my former paper, as pursued by M. Hardy, at a later date, in the wards of the Hospital Saint-Louis. When the affection is in its earlier stages, and the attack not severe, and after all the causes which appeared to maintain it have been removed, the treatment is to be commenced by the application of spirituous lotions, either tepid or hot; a teaspoonful of the following solution being added to a glass of tepid water, and applied night and morning :—

℞. Hydrarg. bichloridi, 1 part; alcohol q. s. ad solvend.; aquæ distillatæ, 100 parts. Misce.

M. Ferrat declares that certain forms of acne, especially the punctated, and one obscurely designated sebaceous, may be certainly cured by the application of astringents locally, and advises the use of alum and peroxide of iron, in the following formula, increasing their strength when necessary :—

℞. Aluminis, 30 parts; aquæ, 300 parts. Misce. To be used as a lotion night and morning.

℞. Ferri peroxidi, 1 part; axungiæ, 60 parts. Misce. To be applied at bedtime.

When the case is more severe, protiodide of mercury is to be substituted for the iron, increasing the dose according to circumstances, to double the strength. Should the cure be long delayed, or imperfect, the biniodide is to be employed instead; and if the attack be very intense, then the biniodide is to be used in the first instance.

I am not sure if my investigations have led, even in a small degree, to the fulfilment of the expectation entertained in the commencement of my undertaking, namely, that of promoting speed and facility in the cure of acne; but it is quite certain that many proposals for treatment have presented themselves to me, clearly of an opposite character. To the plan, which I cannot resist detailing below, on account of its amazing redundancy, I can hardly apply a stronger opinion than that it is simply a project of treatment, for it must appear very doubtful if any unhappy patient could be found willing to submit to its practical application. However, it is put forward as a mode of cure, and it shall speak for itself. At the same time I do not feel inclined to make myself responsible for the definition of the forms of disease named, more particularly as they seem to apply rather to gutta rosacea and syphiloderma.

In the Gazette Médicale de Paris for 1862, Dr. Hedenus of Dresden, giving clinical instruction "*sur l'acne rosea*," meaning thereby *red nose*, says that all the world knows that acne is one of the diseases most rebellious to treatment. His mode of practice, for simple redness of the nose, commences by the following medicine, administered internally:—

℞. Carbonate of soda, ℥iiss.; to be dissolved in eau de melisse, ℥vj.; add extract of orange-peel, ℈iv.; and take three spoonfuls every day.

As an external application, to smear over the surface of the nose, the following is to be used:—Laudanum, 47 drops, mixed with 4 grains of extract of belladonna. The nose is to be well washed in the morning, and small pieces of linen dipped in cold water are to be applied several times a day. When this has been pursued for some time, the following pills are to be taken, eight or ten of them, twice a day; the formula is:—

℞. Sodæ bicarbon, ℈v.; pulvis rhei, ℈iiiss.; ipecacuan., gr. iij.; extracti coloc. co., gr. xv.; extracti chelidon., maj. q. s. M. Fiant pilul. xciv.

If the patient be not relieved at the end of six months, then a mineral alkaline water should be tried, such as the baths of Marienbad or of Homburg ; and this treatment, if necessary, is to be continued during three years.

If the redness depend in any degree upon a scrofulous diathesis, whey mixed with vegetable juices, muriatic baths, &c., will prove serviceable, while, as an external remedy, the following ointment is to be applied :—

℞. Axungiæ, ℥iss ; zinci sulphatis, gr. ij. ; extr. thebaicæ, aquos., gr. iv. ; extract. conii, gr. viij. M. To be spread upon linen and applied alternately with cold fomentations.

During the winter a spoonful of a mixture is to be taken three times a day, compounded of muriate of baryta, and essence of hemlock, dissolved in cherry-laurel water. At the same time one of the following lotions is to be used for external application :—Solution of acetate of lead, laudanum, tincture of benzoin, alcohol, and elder-flower water ; or, solution of lead, acetate of ammonia, and water.

During the summer time the waters of Ems in particular are to be used.

If the cause of the *red nose* depends on menstrual or hemorrhoidal disorder, foot-baths containing nitric acid, and frequent applications of derivatives to the inferior extremities, are useful as external means, while the pulp of potato or scraped carrot may be applied directly to the nose.

At the same time there are to be taken in the morning two spoonfuls of an electuary, composed of agrostis, taraxacum, chelidonium, and millefolium, with a glass of Seltzer water.

The reporter, Dr. Goeschen, exclaims :—"This complicated medication, in which are seen to figure so may remedies, and the mineral or thermal waters of Marienbad, Carlsbad, Homburg, Ems, &c., is appropriate for an affection designated by the author as *simple red nose*, acne rosea.

Now let us examine the mode of treatment advised for acne rosacea, *la couperose*. I must not, however, enter upon so much detail, for the sake of space, as in the previous case, and it will be enough to sketch the plan proposed loosely.

In the first place a liniment, composed of extract of belladonna, extract of opium, ointment of nitrate of mercury, and olive-oil, is to be spread upon the affected parts, by the aid of a pencil or small piece of linen, and allowed to remain for half an hour. If this prove unsuccessful, a strong solution of corrosive sublimate is to be applied for a quarter of an hour two or three times a day, after the plan of Hebra. While this is being used, a very efficacious remedy—muriate of zinc, muriatic acid, and water—is to be taken internally as a mixture; or the muriate

of zinc may be taken with extract of myrrh and extract of guaiacum, in the form of pills, according to circumstances. In case the liver should become very irritable under this treatment, cod-liver oil is to be given in large doses, up to nine spoonfuls a day, while the same oil is to be applied externally.

Dr. Hedenus also recommends iodide of potass, combined with extract of hemlock, in progressive doses, commencing with 2 grains twice a day, increased every day by 1 grain, until a dose of 16 grains twice a day is reached, and then the quantity is to be diminished in the same progression until the original dose is attained. As a local application, ointment of corrosive sublimate is to be applied on linen during the night.

Should all these means fail, it is advised to commence afresh with the baths, a few new localities being added to those above; such as Töplitz and Aix-la-Chapelle, taking at the same time pills composed of crude antimony, extract of walnut, and powder of bitter-sweet, and spreading upon the skin, with a camel-hair pencil, a liniment of oxydulated acetate of mercury, glycerine with starch, recent butter of cacao, and oil of almonds well mixed together.

Such is the plan advocated by Dr. Hedenus; but he appears still to have considerable doubt as to its sufficiency; and thus, after all these medicines, mineral baths, and years of medication may have proved unsuccessful in curing the disease, I think I cannot be rash in adopting the *dictum* that acne, regarded from this point of view, is really "one of the diseases most rebellious to treatment."

The treatment of the most recent time is of a very systematic character, and designed to correct constitutional disarrangement. Its principle is most concisely set out in Mr. Erasmus Wilson's *Student's Book of Cutaneous Medicine*, and consists in regulating the functions of digestion and secretion, by bitters, mineral acids, and chalybeates, and by employing the ferro-arsenical solution in special conditions of nutritive debility. The torpid and weakened tone of the skin is to be remedied by hypochloride of sulphur ointment, rubbed upon the eruption at bedtime, followed next morning by thorough washing with soap and water. Bichloride of mercury dissolved in spirits of wine, or bitter almond emulsion, preceded by juniper-tar soap and water, is another valuable remedy recommended by the same author.—*Journal of Cutaneous Medicine, July* 1867, *p.* 158.

77.—CARBOLIC ACID IN SMALL-POX.

By Dr. Horatio Yates, Kingston, Canada.

[The following case is that of "a beautiful young lady," who was seized with small-pox in a severe form whilst passing through Kingston.]

The first object, after saving the patient's life, was to prevent disfigurement. The ward, of which she and the nurse were sole occupants, is large, lofty, and thoroughly ventilated. The temperature throughout was maintained at 64° during the day, and 58° at night. The room was darkened, and a candle only used for light. The usual general treatment was adopted—viz., animal broths, bread, &c., for food, four ounces of port wine daily, and a quarter of a grain of morphia in half an ounce of brandy at night. On and after the ninth day of the eruption, the quantity of wine, morphia, and brandy was doubled; Gregory's mixture was also given when necessary.

But I am induced to transcribe as briefly as possible the local treatment, its success having been so gratifying. We have for years expected to prevent pitting of the face by excluding light and air from its surface. For many years I have habitually used nitrate of silver, tincture of iodine, mercurial ointment, &c., to the face for this object, with variable success. In this case I employed an ointment—of carbolic acid, two drachms; mutton suet, two ounces, and coloured with lampblack. This ointment was spread thickly upon black cotton wadding, which was applied over the face and forehead, holes being cut for eyes, nostrils, and mouth. This mask was changed every second day, and the face gently washed with soap and warm water, and then including the whole body, with warm water impregnated with carbolic acid.

From the commencement of the eruption no unpleasant symptoms occurred. There was none of that intolerable itching, and no secondary fever—the suppurative fever—ever made its appearance, and on the separation of the last scab with the removal of the last mask, I was delighted to find that not a single pit had been produced.

I am persuaded that the carbolic acid diminished the amount of the suppuration and its duration, and that it prevented the secondary fever. Mr. O'Sullivan, the intelligent house-surgeon, smeared daily one arm of the patient with the ointment. The contrast in the character of the pocks on the two arms was remarkable. The pustules on the arm operated upon were not nearly so full, and the scabs fell off earlier than those on the other arm.

Whatever part the carbolic acid played in this case in preventing pitting, or how much the exclusion of light and air had to do

with it, I cannot say ; but this we do know, that carbolic acid is a powerful disinfectant, and that it destroys microscopic vegetations. It certainly prevented the peculiar odour of small-pox in the room, and, as I have already said, I attribute the absence of secondary fever to this agent.—*Lancet, Jan.* 25, 1868, *p.* 151.

78.—ON THE USE OF CARBOLIC ACID IN BURNS.

By William Pirrie, Esq., Professor of Surgery in the University of Aberdeen.

[Having read Professor Lister's paper on the use of carbolic acid in wounds, it occurred to Mr. Pirrie that it would prove of great service in cases of burn, and he determined to try it on the first opportunity. This soon occurred, the patient being a girl eleven years of age, who had been scalded severely on the face, neck, and one shoulder. She was in great pain.]

Two folds of surgeon's lint, dipped in a liniment of one part of carbolic acid to six parts of olive oil, were closely applied to the whole of the scalded surface, a double layer of tinfoil was placed above the lint, and the whole secured by means of a bandage. The air was thus completely excluded ; and in ten minutes, the patient, much to my surprise, stated that she was free from pain. On the second day after the accident the skin was greatly improved, and the bullæ seemed withering away. On the twelfth day the skin was everywhere perfectly healed, the cuticle having been thrown off ; and, although the case was watched with the utmost care, not a single drop of pus was discovered.

The sudden and perfect subsidence of the pain, the withering of the bullæ, the complete healing of such an extent of scalded surface without the slightest suppuration, in a patient of feeble constitution and greatly depressed by shock, appear to me to show that carbolic acid is well deserving of trial in burns of the first and second degree. I have seen some scalds of less extent prove fatal, and I have seen many not so unpromising at first end in suppuration and ulceration of skin, and require months to heal.

For making clear the state of parts in the first and second degrees of burns, I have been in the habit of stating to our students that they are precisely the same as those which usually result from the application of a blister ; the inflamed part, without separation of the cuticle, resembling the first, and the part covered by vesicles and bullæ resembling the second, degree of burn. This resemblance of the states of parts in blistered surfaces to those in the first and second degrees of burns suggested

to me the desirableness of trying the effect of dressing a blistered surface with the above-mentioned liniment of carbolic acid and olive oil. This was done in the following case, which was carefully watched and recorded by Mr. Nicoll, one of the most talented of our students, who at the time was doing duty for our house-surgeon.

Helen W., aged 27, was admitted under my care on the 27th of August, in consequence of an injury to her right hip-joint, caused by a heavy sack of meal having fallen upon her from a great height. Some subacute inflammatory action took place in the joint; and, after other treatment, a blister of the size of the hand was applied, on the 4th of September, to the outside of the hip, and allowed to remain for seven hours till vesication to a considerable extent was produced, when it was removed; the sensations of the patient and the condition of parts being precisely the same as in burns of the first and second degree. The blistered part was dressed in every respect as in the preceding case of burn, and in fifteen minutes the pain entirely ceased. On the morning of the 5th of September the blistered surface and bullæ presented the same appearance as on the removal of the blister; on the evening of the same day the smaller bullæ had begun to disappear. On the evening of the 6th the smaller bullæ were gone, and the larger greatly diminished; and on the evening of the 8th of September the parts were perfectly healed, no suppuration having taken place.

In both cases carbolic acid seemed to accelerate the subsidence of local symptoms, to procure rapid and perfect relief from pain, and to promote healing without suppuration. If in other cases of the same degrees of burn carbolic acid should be equally useful, it would be a great blessing in the treatment of these dangerous and painful injuries.

Although water may answer for diluting carbolic acid, when used for some purposes, oil must be preferable in cases of burns. —*Lancet, Nov.* 9, 1867, *p.* 575.

79.—CARBOLIC ACID IN THE TREATMENT OF SCALDS OR BURNS.

By Dr. Allan Wilson, Castle Eden.

As carbolic acid in surgery is justly occupying the attention of the profesion at the present time, it may be interesting to some of your readers to know that I have recently used it with marked benefit in cases of severe scalding. The way in which I apply it is as follows :—

1st. To thirty pints of the ordinary oil and lime-water add one of the acid, and mix.

2nd. Saturate linen rags in the carbolic emulsion, and spread them on the scalded parts.

3rd. Keep the rags moist by smearing them over frequently with a feather dipped in the emulsion.

4th. Cover the application with oiled-silk or gutta-percha tissue.

The advantages of this mode of treatment are twofold, as was well exemplified in two cases I have just brought to a favourable termination, one of which was of a most serious nature, involving the face, neck, and chest of a child two years of age.

1st. It excludes or destroys the noxious elements of air, and in this way will diminish shock by removing the cause of local irritation.

2nd. The sores are allowed to heal rapidly by a natural restorative action, without the formation of pus, consequently preventing a most frequent secondary cause of death in burns—namely, purulent infiltration of the lungs or other internal vital organs, and also in a great measure preserving the personal appearance of the patient—a matter of no small importance to some people.

Case 1.—Mrs. M., aged 40, on the 20th November last got her forearm severely scalded with hot tea. The ordinary oil and lime-water was applied at the time by herself, and continued for two or three days, when she got alarmed by the arm becoming much swollen and very painful, and sent for me to see it. I gave her full directions how to apply the carbolic acid treatment, which she attended to strictly, and to her great astonishment the arm was well in less than a week.

Case 2.—C. E., aged 2 years, the daughter of a farmer, on the 23rd of December last slipped from a stool on which she was standing, and, in attempting to prevent a fall, pulled a pailful of boiling hot water off the table on to her face and bosom. The epidermis of the breast and shoulders came off for the most part with the clothes, and that of the face and neck was raised in blebs. On first seeing the case about half an hour after the accident occurred, it seemed from the extensive injury and extreme state of shock, amounting almost to coma, that the case was a hopeless one. The antiseptic treatment was at once applied, and in four hours afterwards the child was quite conscious and cheerful. From this time she steadily improved up to the 27th, when she suddenly got very weak, and was said to have had a slight rigor, doubtless caused by the temperature of the room being allowed to get low, and also by being put on low diet. Ordered one teaspoonful of brandy in water every four hours, and raw eggs in milk. On this regimen she rapidly improved, and at the present date she is running about almost as well and pretty as she was previous to the accident. All the

medicine administered in this case was four grains of mercury with chalk, one of rhubarb powder on the third day.—*Lancet, Feb.* 1, 1868, *p.* 181.

80.—A NEW METHOD OF USING SILVER WIRE IN THE TREATMENT OF WOUNDS AND IN OPERATIONS.

By Dr. S. Wordsworth Poole, Aberdeen, Surgeon to the Cray's Paper-Mills.

Being unaware that silver wire has been employed in the following manner in any of the hospitals, I venture to think that the plan is not much known, though it seems so simple and obvious that it must have occurred to, and probably been practised by, many.

A very common accident in paper-mills is the vertical amputation of one or more fingers, through about the middle of the last phalanx, by the paper-cutting machine. In such a case sutures are not available, an operation is hardly called for, and by the ordinary treatment with water-dressings or stimulating lotions, a month elapses before a perfect skin is formed; but by the following means a perfect cure is obtained in, at the farthest, fourteen days:—A slip of adhesive plaster being taken, about as wide as a finger-nail, four loops of wire are attached to it. They are to be so placed that when the plaster, heated, is applied firmly at a little distance from the truncated extremity, four free ends shall project beyond the finger on the dorsal aspect, and four on the palmar. A larger piece of plaster having been applied so as to give additional fixation, the opposite wires are twisted over the wound, two and two, by a dressing forceps, as much as seems advisable. Thus the exposed surface is considerably diminished in size, and a quantity of blood and lymph is forced out, which forms a firm coagulum that does not decompose, and remains until it is found that a new and perfect skin is formed underneath.

The wire may be similarly used in flap operations on the fingers and the great toe, and I have found it much superior to plaster in fastening a lacerated finger to a splint. The opportunity has not occurred to employ it thus in a larger operation; but it seems to me that a modification of it would be useful wherever the circumference of the limb could be embraced by the strip of plaster. The first application is undoubtedly troublesome, but, as a rule, no further interference would be required, and it would not prevent the use of those antiseptic dressings lately come into vogue.—*Lancet, May* 9, 1868, *p.* 588.

81.—ON THE TREATMENT OF PRURIGO.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Diseases of the Skin.

In all forms of this justly-dreaded affection, the remedies which seem to have succeeded best resolve themselves pretty well into six classes.

1. *An alkali.*—Soda, for instance, seems to have always been a favourite ingredient in the prescriptions of the most successful practitioners. I do not attribute much importance to the form in which it is given; perhaps the carbonate, or a mixture of carbonate and sulphite, answers best. If there be much indigestion or acidity, ten grains may be given two or three times a-day, just after meals, either in an ounce of bitter infusion, with which a little compound tincture of cinnamon or spirit of nutmeg is combined, or it may be administered in conjunction with aromatic confection and dilute hydrocyanic acid. These remedies should be continued till indigestion is removed; should no indigestion be present they may be given up at the end of ten days or a fortnight.

2. A remedy is required which will act on the skin. Sulphur in some form or other is perhaps the mildest and most certain. Antimony, however, in such a preparation or dose as will not nauseate or depress, may also be prescribed with benefit, and I see no objection to combining these remedies with a little nitrate of potass. When the skin is very dry and harsh, as it almost always is, a powder containing these ingredients may be given on going to bed, in a little gruel or warm whey. Like the previously mentioned remedies, these need not be continued more than about a fortnight

3. The use of either of these two sets of remedies should be accompanied by a course of medicine which will act gently on the liver and bowels, as, for instance, the occasional use of small doses of iodide of potassium, with rhubarb, either in the form of infusion or pill. A little mercury may be given in the shape of blue pill, combined with soap, henbane, and jalap or colocynth; or as grey powder, with nitrate of potass. Mr. Startin places mercury under interdict, though I have never seen it do the least harm.

4. But the great internal remedy in prurigo is arsenic, and in all very severe or long standing cases, and in those which do not seem to be benefited by the preceding treatment, I would advise that it should be immediately begun with; and the other remedies used only as auxiliaries, or in the place of the arsenic when it is necessary to interdict it. It may be prescribed in full doses, and when this is done, I am disposed to share Mr. Hunt's opinion that prurigo is not such an intractable disease if

properly treated, although I am aware that the statement may be looked upon as a modern heresy, and that Mr. Wilson considers it a stubborn and even a grave complaint in elderly persons. Under the old plan of treatment it seems to have resisted every attempt to subdue its malignity, and, perhaps, there were more suicides from prurigo than from all other diseases of the skin put together. Mr. Hunt says—"Of the entire recovery of a patient thus affected, or even of considerable alleviation of suffering, not one single gleam of hope can be gathered from any author who has written on the subject. And yet there is no truth in the whole circle of medical science more vividly impressed on my own mind, than that, under proper management, arsenic is an effectual remedy for this disease." Mr. Hunt bleeds to faintness in refractory cases before giving the arsenic; in one case he took about seventy ounces of blood from the arm, and probably fifteen additional ounces by leeches, and certainly, according to his statement, the success seems to have quite justified the means. Lisfranc used also to bleed in cases where the itching was connected with difficult menstruation, and the practice is said to have been very beneficial.

5. Certain remedies which seem to stimulate nutrition in some as yet unknown way. Of these two are well worth a trial —strychnia and cod-liver oil. The strychnia may be given in doses of a sixtieth or a sixty-fourth part of a grain every three or four hours till a decided effect is produced upon the disease, or till nervous symptoms show themselves, when it may be left off. It sometimes acts like a specific upon the pruritus, and when this symptom is once thoroughly quelled, we shall seldom, if ever, have much difficulty in dealing with the remaining symptoms. Nux vomica was a favourite remedy with Neligan, but I believe we are indebted to Dr. Burgess for this mode of exhibiting its active principle. It should not be taken along with any other remedy, whereas the cod-liver oil may be given almost as an article of diet for a long time, and in moderate doses, quite irrespective of any other medicine the patient may be taking. I confess my entire ignorance as to how cod-liver oil and strychnia may act, and it may, perhaps, save some trouble if I say, once for all, that I am not prepared to offer any explanation of the action of medicines, for the simple reason that in respect to most of them nothing whatever is known for certain, and as to cloudy conjectures and cabalistic forms of speech, I leave them to those who like them. I am content to admire at a distance the person capable of solving such questions as causes and modes of action, as I quite despair of my own ability to do so.

6. A free use of hot baths, especially the Turkish bath. I

prefer the latter when it can be used, but when the patient is very nervous, suffering, or supposed to suffer, from disease of the heart, or subject to fainting; where he resides at a long distance from any establishment or baths of this kind, or where expense is an object, the hot bath is our only alternative, and fortunately it is a very good substitute. The water should be at quite ninety-eight or a hundred degrees of Fahrenheit. The patient, so soon as he enters the bath, should scrub himself all over with the flesh brush, lather the affected parts with either the soft soap of the London Pharmacopœia or Pears's transparent soap, and then let himself down into the hot water, in which he ought not to remain more than three or four minutes. Having dried himself thoroughly, he should apply any ointment he may be using and dress directly.

But the Turkish bath is the thing; it scarcely ever fails to do good, and is, perhaps, more peculiarly suited for prurigo than for any other disease of the skin. There are many persons in the habit of sponging all over daily, and who, therefore, have quite made up their minds that they require no other kind of purification. They cannot too soon be undeceived as to the efficacy of water used in this way; it will no more free the skin from soot, dust, dead scarf skin, and secretion, than rubbing a horse gently down with a soft towel will make his coat glossy. To rid the surface thoroughly of these impurities, two things are necessary: free perspiration must be induced, and the skin must be well rubbed and kneaded. Trainers are well aware of this, and when preparing a man for a fight, make him perspire freely, and then rub him down with a hard towel. The vigorous system of cleansing adopted in a Turkish bath is more like that used in training than any other, and will soon open the eyes of those who put their faith in cold sponging only, by bringing away an unexpected quantity of dirty skin. As there is no danger to be apprehended from the use of the bath, so long as the simple precaution is adopted of not staying in too long at first, and always taking the bath on an empty stomach, it can scarcely be overdone. I have known one taken three or fours days together without any harm arising from it, and should not anticipate any.

Some most extraordinary objections have been made to it. Mr. Hunt, for instance, expatiates with grim sarcasm on the portentous aspect of things on entering a Turkish bath—the air so hot as to make one fancy that it cannot be breathed without setting the lungs on fire, the kneading and trampling on the patient, and the drowning with a deluge of cold water, but he admits that it cures fanciful people of their whims in superlative style. Then one of the whims it will cure them of is fancying there can any harm come from using the bath.

Whichever form of bath be adopted, I would strongly insist upon the necessity for taking it regularly and often enough, and not yielding to any nonsense about baths being lowering, weakening, &c. I dwell upon the subject because the advantage of hot bathing in prurigo generally so soon becomes manifest. If nothing else be gained, free perspiration is promoted, and though this is no panacea, yet it is attended with relief. Prurigo often breaks out in persons who have nothing to reproach themselves with in regard to cleanliness. Mr. Startin, indeed, says, "that the neuralgic itching which some writers consider as a form or variety of prurigo is, perhaps, more frequently met with in the respectable waks of life than any other cutaneous affection." Such persons often seem surprised at being told to make free use of hot baths, but they forget that they do not take sufficient exercise to keep the skin in a healthy state, and that means which would do very well with men riding twenty miles a-day, or working hard at training, are quite inefficient when exercise is reduced to a gentle stroll. Elderly persons in good circumstances, and people who have retired from business, often seem to think it is hardly respectable to go beyond a steady walk, but prurigo will not yield to such gentle means, and till regular active exercise has become a settled habit, the action of the skin must be encouraged. In all cases I think no woollen ought to be worn next the skin.

Mr. Wilson gives generous diet and tonics in this disease, and considers arsenic, properly given and watched, as a specific. Frictions, baths, carbolic acid soap, and juniper-tar soap, are his chief local remedies, but he looks upon the prognosis as doubtful, on account of the exhaustion and suffering which accompany the complaint.

Mr. Startin's treatment of prurigo is that of lichen; he relies chiefly on mineral acids, chalybeates, opium, and ammonia, and has never found benefit from mercury or arsenic; on the contray, they rather do harm. In a very severe case of prurigo formicans, recorded by this gentleman, twenty drops of dilute sulphuric acid and ten of Batley's sedative three times a day, followed by tincture of muriate of iron, in infusion of quassia, and an opiate at bed-time, proved perfectly successful. These means were, however, seconded by the use of an ointment of of white precipitate of mercury and creosote, ten grains of the salt, and a few drops of the fluid to an ounce, and after this a weak solution of bichloride of mercury in creosote water, used warm; later on, the ointment was entirely discontinued, and bisulphuret of mercury was added to the lotion. A strict diet, consisting of milk, bread, and boiled meat was observed. Mr. Startin finds hot air baths and cinnabar fumigations of great service. Dr. Neligan used to prescribe iron in infusion of hops,

with the juice of conium in pretty large doses. He found in the prurigo of old people great benefit from the use of this remedy along with magnesia. Some years ago, Dr. John Waterfield communicated to the Lancet and Medical Gazette a paper on the value of tar and charcoal pills, and he now tells me that he has treated several cases of prurigo very successfully with this remedy. Dr. Hillier says that in some chronic cases diuretics, "such as the sweet spirits of nitre, decoction of broom, with the compound tincture of juniper and saltpetre," are of benefit. Dr. Purdon communicated to the Journal of Cutaneous Medicine three cases in which the bromide of ammonium in doses of from ten to twenty grains effected a very rapid cure.

Hebra, who only admits prurigo mitis and formicans, views the malady in its gloomiest light; the picture is filled in with the most sombre tints that utter despair of being able to do any good can lend. The patient is doomed *tenero de ungne*. The relentless malady begins with childhood; even in his schoolboy days the martyr to it is an outcast; playfellows and masters alike shun and worry the victim of unappeasible itching. As he grows up, he is expelled from society or becomes a recluse; if he be a working man, he must not sleep in the same room with a fellow workman. He dare not try to establish a home, nor, should he already possess one, can he bring a wife to it. With advancing life the disease acquires more hold on the system, and only leaves him in the grave. Till that hour comes he knows no hope and no peace. The complaint is not difficult to cure, for the simple reason, that it is incurable, and all that the physician can do, is to make the patient's condition a little more tolerable. Hebra seems never to have given arsenic a fair trial, and it may be safely predicted that so long as he does not, he will find the complaint as refractory as he describes it to be. He utterly abjures bleeding, purgatives, and starvation, possibly with reason, but in point of fact I know nothing of the disease as spoken of by him, and never read of it in the pages of any other writer. Possibly in this severe, hopeless form it is as peculiar to Austria as pellagra to Italy.

In some cases, particularly when prurigo attacks the pudendum, scrotum, or anus, the itching is so intolerable that something must be done locally for it; indeed the itching is the symbol of the malady, the one essential and tangible symptom, and the cure of it is the cure of the disease. Mr. Wilson recommends, in a general way, that the skin should be frequently rubbed with a damp sponge, dipped in fine oatmeal; after this, the tincture of croton, made by steeping an ounce of bruised croton seeds for a week in four ounces of spirit, is applied, and after this has been done a few times, a lotion of bichloride of mercury in almond emulsion, fifteen or twenty grains to a pint,

will often prove very efficacious. Painting the surface with iodine is useful, as is also glycerine applied with a sponge. For the affection of the pudendum, Bateman recommends a lotion made of two grains of bichloride (oxymuriate) of mercury in an ounce of lime water. Mr. Wilson says the juniper-tar ointment is peculiarly valuable in this variety. In pruritus of these parts injections of very hot water, juniper-tar ointment, blisters to the thighs, and small bleedings, seem, from all accounts, to be the most reliable means of cure. Mr. Wilson has found an opium injection relieve the irritation after all other means had failed. Strong nitric oxide of mercury ointment, and podophyllin in doses of one-sixth of a grain, are said to have proved of great service in prurigo of the anus. Mr. Startin, in a case related in his lectures, where the scrotum was also affected, directed mucilaginous hip-baths, daily ablutions with yolk of egg and tepid water, the application twice a day of very dilute mercurial ointment, with a few minims of creosote, strict diet, and one sixth of a grain of bichloride of mercury in cold infusion of hops three times a day, The case was very severe, and occurred in an old man; but a cure was effected in little more than two months. In the prurigo of old people generally, Mr. Startin's prescription for external use is a liniment of glycerine and trisnitrate of bismuth or powdered talc, rubbed in with a flesh brush. Dr. Frazer recommends for trial finely powdered camphor mixed with six or eight parts of rice or potato starch, and a small quantity of acetate or carbonate of lead. This is dusted on the skin three or four times a day, its action being aided by calomel ointment. Latterly, Dr. Neligan confined himself almost entirely to chloroform ointment, which seems one of the best, if not the best ever introduced. It is made by mixing half a drachm of chloroform with an ounce of cold cream. I believe this and the following lotion are two of our most valuable remedies.

℞. Hydr. bichlor. gr. iv.; bismuth oxyd. ʒss.; acidi hydrocyan. dil. Ph. Lond. ʒss.; aq. calcis ad. ℥viij. M. To be applied warm two or three times daily.

Mr. Balmanno Squire considers that prurigo senilis is always due to pediculi, and we may naturally expect to find that his principal reliance is upon external means. That in some persons pediculi will induce symptoms which might very easily be mistaken for prurigo is possible enough, but that they ever bring on genuine prurigo, a disease in its severer form so generally associated with some deep-seated constitutional disorder, is a very different matter. Mr. Naylor, who thinks the insect cannot produce any form of this complaint, says he has known a very weak nitric acid lotion—half a drachm to eight ounces of water or one of bismuth, a scruple to six ounces of water,—prove

exceedingly serviceable. He also says that when prurigo is an idiopathic affection the use of chloroform is often of great service, applied either in the form of vapour or of an ointment consisting of equal parts of chloroform and camphor liniment.—*Medical Press and Circular, March* 11, 1868, *p.* 219.

82.—ON PRURIGO.

By Dr. Henry Purdon, Physician to the Belfast Dispensary for Diseases of the Skin.

The following three cases of prurigo have been selected for the purpose of showing the value of the bromide of ammonium in this distressing complaint.

1.—Mrs. M., aged 60, admitted at the Belfast Dispensary for Skin Diseases, in February, suffering from prurigo, and which has existed for upwards of six years, is always worse at night. Formerly was in better circumstances, and attributes the disease to mental anxiety and bad food. At present (February) her arms and chest are excoriated, presenting all the typical appearances of prurigo; the cuticle is also discoloured.

Various remedies were tried in this case, both locally and constitutionally, such as lotions containing hydrocyanic acid, borax, and morphia, the latter being administered by hypodermic injection, and from the use of which most benefit was derived, but no permanent results were obtained. At the end of February I determined to try the effect of the bromide of ammonium, commenced in doses of ten grains, three times a day, gradually increased to twenty; and discontinued all local treatment. After taking the bromide for about a fortnight the feeling of formication disappeared, the patient obtaining sleep at night. The diet consisted principally of farinaceous food and milk. At the present time (July) she is perfectly well.

2.—J. M., aged 64, formerly a butler, admitted at the Dispensary for Skin Diseases, May 1st, suffering from prurigo, which has existed for about two years; was formerly much given to drinking whisky, to which he attributes the present disease; appetite fair; occasionally sleeps well at night; bowels usually costive. In this case the treatment was commenced by administering a pill containing podophyllin, pil. colocynth co. and ext. of Indian hemp, every second night for about a week, so as to get the liver and bowels to act naturally, after which the bromide was commenced, continued steadily till June 29th, when he was discharged cured.

3.—H. S., aged 59, admitted into the infirmary of the Bel-

fast Charitable Society, under my care, on February 4th, suffering under a severe attack of prurigo; has always been temperate, but is greatly debilitated; no appetite, and his mind wanders occasionally. An extensive eczematous eruption exists on his chest, arms, and back, arising from the patient tearing and scratching himself to relieve the excessive itching. In this case quinine was prescribed, and a lotion of sulphate of zinc, glycerine, and water, as an external application, with twenty drops of liquor morphiæ hydrochloratis at night to procure sleep. In the month of March, as no benefit was derived from the above treatment, and from the success attending the administration of the bromide of ammonium in case 1, I determined to commence its use, its effects being apparent in a few days, the patient expressing himself much better. The only local treatment adopted was the application of calomel ointment (a drachm to the ounce of lard), under the use of which the eczema disappeared, and, as the itching was greatly moderated, he did not feel the same inclination to tear himself. By the end of April the patient was quite well of the prurigo, but several carbuncles made their appearance on his neck, some of which quickly suppurated. The treatment now adopted was, on the first appearance of the carbuncles, pressure, and when it was evident that suppuration must take place, a poultice for a few days, after which they were opened, a yeast poultice being then applied: at the same time a generous diet and wine was ordered.

The patient's constitution being evidently quickly breaking up, and as he made very little urine, and that high-coloured, I prescribed a stimulating diuretic, which treatment I believe is recommended by Dr. Day for prurigo in his work on "Diseases of Advanced Life," but from which no benefit was derived. Considering his present complaint to be due to a blood poison, I ordered the bisulphite of soda in infusion of cinchona: this certainly prevented the appearance of any new carbuncles; but a large anthrax having already formed on his back, from this he rapidly sank.

The above case is, I think, interesting, but I shall leave it to those whose experience is more extensive than mine to show the connection between the two diseases. I shall only mention that I have frequently seen an eruption of furunculi make their appearance after prurigo has been relieved. Could prurigo depend on a blood poison? for this disease is often associated with jaundice, gout, rheumatism, and Bright's disease.

Since the above cases presented themselves I have treated several others with the bromide of ammonium with marked success. The only local application that I use is borax and glycerine, and that merely as a *placebo*. The bromide, when first administered, quickens the pulse, and when given in large

doses is a powerful anodyne, occasioning sleep and increasing the secretion of urine.

According to Dr. A. T. Thompson, prurigo frequently arises from the abuse of fermented liquors, as also indigestible kinds of food, and is, I believe, now looked on as a neuralgia of the papillæ of the skin. Many dermatologists consider it to be evoked by heat, and when constitutionally recurrent is a form of eczema, to which the name eczema papulatum has been given. Dr. McCall Anderson considers prurigo to be a form of lichen, and "consequently an eczema, the papules being identical with those of lichen, the black crusts being produced by scratching." Pruritus of the anus frequently arises from engorgement of the liver and ascarides in the rectum.

Cazenave considered prurigo to be a disease of sensibility, depending on the retention of some of the constituents of the urine in the blood. Pediculi often occasion the disease in old people, from the irritation of which the patients tear and scratch themselves, a papular eruption being produced.—*Journal of Cutaneous Medicine, Oct.* 1867, *p.* 326.

83.—LARGE CYSTIC BRONCHOCELE CURED BY IODINE INJECTIONS.

By T. B. Curling, Esq., F.R.S., Surgeon to the London Hospital.

Emily G., aged 54, a widow, was admitted into the hospital on February 20th, her home being at Long Milford, in Suffolk. About fifteen years previously she noticed a swelling towards the left side of the thyroid cartilage. It grew very slowly; but about five years before admission its rate of growth increased very much; and when first seen on February 20th, the tumour, which was smooth, and fluctuating towards its anterior and lower part, occupied the lower two-thirds of the anterior triangle of nearly all the posterior, causing the circumference of the neck opposite the most prominent part of the tumour to measure 19 inches. Towards the outer and posterior part an artery, apparently equal in size to the external carotid, being quite superficial, could be seen and felt running upwards towards the angle of the jaw; numerous small veins were also visible. The swelling ascended when the patient swallowed, showing its connexion with the larynx.

On Feb. 25th, the tumour was tapped, and seventeen ounces of reddish serous fluid were drawn off, reducing the size to 16 inches. No constitutional disturbance followed, but by March 16th the size had increased to 18 inches. It was then again tapped (about 14 ounces of dark bloody serum being removed), and

injected with about one drachm and a half of compound tincture of iodine (Ph.) and water, mixed in equal proportions, which merely caused a smarting for about an hour.

The following day the tumour had returned nearly to the size it had before the tapping and injection.

On Friday, March 26th, the tumour, having again increased to 18 inches, was tapped, but only a small quantity of fluid drawn off. It was injected with the compound tincture of iodine undiluted, about a drachm and a half being thrown into the sac, and left there. No pain or uneasiness was felt in it until the evening of the following Monday, when she had pain in the head, giddiness, and much fever; and on the following day the tumour became hot and painful, and measured 19 inches.

In a few days the constitutional disturbance subsided; the tumour being first treated with a poultice, and subsequently ice was applied. Although the general symptoms became less, there was considerable dysphagia, and the tumour increased in size, and on April 12th measured 20 inches, and was very red at its anterior and lower part. She could then swallow only fluids.

April 14th. Tumour (now measuring 20½ in.) was more painful, very hot, and the skin œdematous at the posterior part, which now fluctuates distinctly; no appetite nor sleep; pulse 120. It was now determined to let out the fluid. The sac was first punctured with a trocar, and then opened freely with a scalpel, letting out about twenty ounces of fluid, which afforded immediate relief. The fluid consisted of an opaque liquor, containing altered blood-discs and compound granule-cells. A drainage-tube was then introduced, and in a few days the discharge became purulent, thick, of a greenish colour and offensive odour.

30th. The tube was still kept in, and the discharge was copious. Protruding through the wound there was seen an apparently ordinary slough, but upon withdrawal it was found to be a firm membrane folded on itself—being, in fact, the wall of the cyst. After this the discharge continued, though somewhat lessened in quantity and more healthy in character. The general health was very good. The circumference of the neck was reduced to 15½ in.

June 3rd. Still discharging from the original opening, a little to the right of which there was a prominent fluctuating swelling, which was opened and several ounces of pus let out. During the next four weeks two other small collections of pus became prominent, and were opened, discharging pus.

July 22nd. The general health was now quite restored; there was no dysphagia, and the appetite was good. The circumference of the neck was now only 14½ in. There were three or four small openings in the anterior part of the remainder of the tumour, which was firm and painless. These openings discharged a little thin fluid, and were occasionally touched with nitrate of silver.

She left the hospital on the 30th, the tumour decreasing in size, and being so much diminished as to occasion very little deformity. Two of the sinuses were closed, but one continued to discharge a little. There was scarcely any swelling in the neck, and the patient was discharged cured.

The morbid changes in the thyroid gland producing the swelling called bronchocele are of two kinds: hypertrophy of the natural structure, the common and well-known form of the disease; and the development of a cyst, the case related being a marked example of the latter. Cystic bronchocele is a much rarer form of the disease than hypertrophy of the gland. It occurs at all ages after puberty. The cystic formation is generally single, and it differs further from hypertrophy in being limited almost invariably to one lobe of the gland. When the cyst acquires a large size, the glandular structure becomes obliterated by pressure. The seat of the disease, however large the cyst may be, is rendered obvious by its connexion with the larynx. When the patient swallows, the tumour moves up with the windpipe; and in this way cystic bronchocele is easily distinguished from the cysts formed in the areolar tissue of the neck, called "hydrocele of the neck." A large cystic bronchocele is a serious disease, not only on account of its producing an ugly tumour and great deformity in a conspicuous part of the body, but also from its liability to press on the trachea and œsophagus and to obstruct these passages. Gooch, an old surgical writer, whose works are now seldom read, but who has recorded many interesting and remarkable cases, mentions one case of cystic bronchocele in which the trachea was compressed and contracted by the cyst. Some similar cases in which deglutition and respiration were impeded by a cyst in the thyroid gland, will be found in a paper in the *Achives Générales* for 1837. A cystic tumour in the thyroid gland, however, does not always grow to a large size. Whilst still small, it may remain stationary and cease to increase. Some years ago I removed from the dead body a small cyst in the right lobe of the thyroid evidently of very old standing, for it is encased in bony deposit, forming a complete osseous sac. The specimen is in the London Hospital museum. When the cyst remains small, no surgical interference may be called for; but in the case of Emily G. the tumour

had attained a great size and was steadily enlarging, so that the patient became very anxious for something to be done in order to get rid of it.

The cure of cystic bronchocele may be effected in three ways; by excision, seton, and injection. Dr. Beck, of Fribourg, who published some cases of cystic goître in 1836, treated three of these by excision. Two were cured, one after severe inflammatory symptoms; the third was doing well, when low fever ensued, and the patient died of pyæmia. The circumstance that an artery of great size passed over the outer part of the cyst precluded my thinking of excision in G——'s case. Besides, the disfiguring long cicatrix remaining after excision constitutes an objection to this operation. The seton is an effectual mode of treating this disease. Maunoir, of Geneva, in a memoir published in 1825, gave an account of four cases of large thyroid cysts which were cured by seton; and Mr. Spencer Watson lately showed to the Pathological Society a woman with a large cystic bronchocele, which interfered with respiration and deglutition, and which he was treating successfully in this way. Injections of iodine act so well in the cure of serous cysts that I was induced to try them in G——'s case, especially as the sac was by no means thick. The second injection caused active inflammation, followed by suppuration and separation of the cyst. The result, however, was satisfactory in effecting a cure of the disease. The rapid increase of size after inflammation had set in interfered seriously with deglutition, so that the patient was unable to swallow any solid food. This was quickly relieved by a free opening into the cyst made by the house-surgeon, Mr. James Adams. Suppuration in a cyst of so large a size, and near so many important structures, is not altogether free from danger. There is a preparation in the museum of St. Bartholomew's Hospital of a large cyst of the thyroid gland, which inflamed and enlarged rapidly after tapping, and the patient died rather suddenly, suffocated. The cyst had burst, and discharged its contents into the pharynx and larynx. The same thing happened in Gooch's case. The patient expired suddenly in consequence of the cyst inflaming after being incised, and opening by ulceration into the trachea. Injections of iodine are certainly not so well adapted as the seton to the cure of cases where the cyst is much thickened. There is another advantage in the seton over injections—that it is free from the risk attending the rapid enlargement of the cyst and the confinement of matter, which proved fatal in the cases just alluded to, as there is always an outlet for the escape of fluids. So that, upon the whole, I regard the seton as the more valuable plan of treatment, and the one most generally applicable to these cases.—*Lancet, Dec.* 14, 1867, *p.* 729.

84.—EXTENSIVE ERUPTION OF WARTS: RELATIONSHIP BETWEEN WARTS AND CANCER.

A very extraordinary example of the free production of common warts has been under treatment lately, at the Hospital for skin diseases. The subject is a young clerk of 19, who was sent to Mr. Hutchinson by Mr. Garman, of Bow. His face was covered with warts, and very greatly disfigured. They grew in all parts, but especially on the forehead. The were of the ordinary foliated form, some flat, others slightly pediculated. On both hands are also very numerous warts. On the right hand, they completely surround the nails and lift their edges, though without pain or inflammation, and without disturbing the nutrition of the nails themselves. Mr. Hutchinson remarked on the rarity of any large production of warts in the face; and stated that he had never seen so many as in this instance. He also directed attention to an interesting fact in the patient's history, that his mother had died of "cancer of the womb." He stated that several facts which had come under his observation confirmed the opinion of those (Mr. Nunn and others) who believe that there is some connexion between the liability to warts and the liability to epithelial cancer. The youngest patient upon whom he had ever operated for epithelial cancer, was a man aged 22 with cancer of the penis, and whose hands were covered with large warts of most unusual exuberance.

In this case, the warts have been treated by touching them with the acid nitrate of mercury, and most of those in the face are now cured. A portrait of the lad, showing his original condition, has been kept.—*British Medical Journal, Dec.* 14, 1867, *p.* 549.

SYPHILITIC AFFECTIONS.

85.—A CONTRIBUTION TO THE HISTORY OF VISCERAL SYPHILIS.

By Dr. W. Moxon, Assistant Physician to Guy's Hospital.

The value of the study of morbid anatomy has never had a stronger proof than it is receiving from the service it has rendered, and is rendering, in showing that syphilis causes grave and fatal visceral diseases, which are easily recognised.

When John Hunter wrote—"The brain, heart, stomach, liver, and kidneys, and several other viscera, have never been known to be affected by syphilis," he expressed the truth, as far as it is seen in common experience, of what I may call the *surgical stage* of syphilis; but now-a-days a *medical stage* of syphilis has to be recognised, which had not then drawn special

attention. The labours of Dittrich, who showed the morbid anatomy of syphilitic lesions, deserve to rank with those of Bright, and this not less for the bold, comprehensive, and correct conclusion which either observer drew from his observations than for the frequency and gravity of the disease concerned in either case ; while, in one respect, the demonstration of these syphilitic lesions surpasses in value even Bright's great discovery, inasmuch as a more definite and hopeful plan of treatment is indicated by the discovery of syphilis than by the discovery of Bright's disease of the kidney.

Though the further labours of Wilks, Bristowe, and other observers have done for Dittrich's discovery more than Rayer and Christison did for Bright's, even now I am persuaded that the frequency of syphilitic causation of medical disease is not usually so familiar and ready to the mind as it should be in order to the early detection of it under the very various forms in which it comes clinically before the physician.

Until the doctrine which ascribes to syphilis the lesions in question is universally and fully allowed and put into practice I conceive that those who have considerable means of testing the truth of the view lie under obligation to state publicly their experience. For, as is usual, and as is right, in the rise of new doctrines, there are those who oppose a sceptical face to the new opinions and deny the syphilitic nature of the affection. Such natural and useful doubts expressed by observers of distinction are very powerful to hinder the spread of the recognition of visceral syphilis among those practitioners who have little or no opportunity of personally examining whether anything characteristic of syphilis is in the viscera of cases which have been treated unsuccessfully,—perhaps, because the suspicion of syphilis as the cause of the trouble was never aroused.

The full and universal acknowledgment of the syphilitic nature of these morbid products will not be reached until observers who have large opportunities give their experience equitably, stating all their observations which bear on the question, whether those observations favour the doctrine or any other. One-sided statements will be received with suspicion, for a disease so frequent as syphilis is sure to be very often by chance found associated with other diseases. That a diseased state may be fully recognised as directly syphilitic it must be shown that it occurs in syphilis, but it must also be shown that it does not occur except in syphilis. There must be doubt of the syphilitic nature of lesions which are believed often to occur apart from a history, or from the ordinary manifestations, of syphilis ; and, to put the question into a state of certainty, it is necessary that *all* the cases in which that lesion occurs should

be fairly examined. Each lesion, too, must be separately so examined, or well-founded doubts concerning one may weaken otherwise good evidence in favour of another which is grouped with it.

For example, ths conditions of liver which are set down as syphilitic are :—1. Simple perihepatitis ; 2, simple interstitial hepatitis ; 3, gummous hepatitis ; and 4, the amyloid or lardaceous change. The description of all these as alike syphilitic produces a very inequitable judgment of their relative significancy ; any one will doubt whether cases of thickened hepatic capsule, or of simple induration of liver by fibrous formation, can be regarded as syphilitic, since we all meet these conditions when there is no suspicion of syphilis. But it is quite otherwise with gummous hepatitis. This is practically never found without the presence of other evidences of syphilis. So that it is necessary to discriminate between these four lesions in the gathering of evidence, and to take each on its own merits. We shall, I believe, conclude with more and more certainty that the gummous form is essentially syphilitic ; but as for the capsulitis and fibrous hepatitis, the occurrence of these conditions from spirit drinking, &c., must impede our arrival at proved conclusions respecting their essential connexion with syphilis. A careful inspection of the forms under which capsulitis and fibrous hepatitis appear in syphilitic bodies and an examination of any apparently distinguishing characters by a fair comparison with these lesions as they appear in unsyphilitic bodies are very desirable, and need to be done very carefully. I believe that it will be found that whenever capsulitis with fibrous hepatitis are strictly localised to parts of the liver there will be found other evidences of the presence of syphilis.

It appears to me allowable that I should contribute such experience on this subject as has fallen to hand in Guy's post-mortem theatre during the last two years. This experience, though not all unequivocal, will, I believe, tend further to convince that visceral syphilis is among the most important of the conditions which we have to recognise.

My object is not to advance anything new respecting syphilis, but to increase the number of records of the conditions found in bodies proved to be syphilitic, or in those who have allowedly syphilitic changes present in them. At the same time I think it will be probable that a certain form of renal disease, and, perhaps, certain forms of disease of the lung, and of the Fallopian tubes, are essentially syphilitic. But even if no definite conclusions should come directly from these cases, I would narrate them, that they might compare with other groups of syphilitic cases, for I believe that useful conclusions may follow naturally after many collections of cases are on record, while

those conclusions may be quite unsupported—perhaps scarcely indicated—by any one set of cases. By enlarging the field of observation we may fairly hope to learn other effects of syphilis beyond those we already know.

Every lesion which so becomes recognised as syphilitic forms a new and valuable means of recognising other syphilitic changes. These will be easily discovered in proportion to the constancy of their association with known effects of the disease, and when their taint is recognised they will reflect on their associates and strengthen the whole evidence in doubtful cases.

For it must be allowed that there are sure to be cases truly syphilitic where no account of the syphilis can be got from the history; circumstances often render it imprudent or improper to ask directly after such a disease, and the truth of replies cannot be relied on when the question is put. And, further, even if we could always learn all that the patient knows about his own case, we should find much difficulty from the confusion of the non-infecting sore with the truly syphilitic chancre, so that after the primary disease has long passed by we may be quite unable to learn whether a scarce remembered disease of the genitals was really syphilis.

This dubious quality of the direct history makes it very highly desirable that all the effects of medical syphilis should be well known, so that any one of them that presents itself may be identified by the aid of others which are grouped with it in the particular case. For whatever theoretic views any of us may hold as to the constitutional nature of syphilis, no one would be content to discover only local lesions in a case where tertiary syphilis forms part of the patient's condition; every one will see that syphilitic cachexia is a thing which it is of the first importance to recognise.

Cases of a disorder so diversified in its numerous forms—there is no disease very unlike some form of syphilis—are difficult of arrangement. The foremost aspect of pathological anatomy is that which bears on clinical practice, and hence I think it will be preferable that the cases should be grouped in respect to the organ for whose disorder they came under treatment.

[These remarks of Dr. Moxon are followed by the post-mortem examination of 27 cases in support of his views, which we advise the reader to study attentively. We have only room to give one case from each of the classes mentioned by Dr. Moxon.]

I. Case in which the Nervous System was chiefly diseased.

Case 1.—*Syphiloma of brain with softening; syphilitic disease of mid. cerebral artery; syphiloma of testes.*—Joseph D., aged 29,

admitted September 20th, 1865, into the clinical ward under Dr. Moxon's care. He was suffering from hemiplegia, with imperfection of mind.

He gave some account of venereal disease, but the state of his mind was so imperfect that little could be made of it.

He had been ill about three months. The attack was gradual; hemiplegia of the left arm commenced after a convulsion, and though varying in amount, yet progessed. Meanwhile he had had two other fits; in none of these convulsive fits had he quite lost consciousness; his mind had grown gradually weak, he did simple things, and laughed or cried on slight occasions.

He was a man of middle height, with square features and red-brown hair; he complained bitterly of headache, now frontal, now occipital; his face wore an expression of suffering; his left arm was quite useless; he was regardless, and he passed his evacuations under him.

While in the hospital he had attacks of vomiting, and suffered much from headache; his bowels were natural; the temperature in the axilla was never over 98 degrees; he was noisy and dirty. He fed himself with a spoon with his right hand to within a few days of his death, but then that hand also became very feeble. He died rather suddenly as if choked.

On inspection the body was considerably wasted; the left foot dropped at the ankle; there was no bedsore; the pupils of the eyes were equal.

Head.—On raising the dura mater the hinder part of the right brain hung away down like a bag of liquid, and the removal was difficult from fear of breaking this. In the diamond-shaped space the membranes were opaque, and the basilar artery had lymph around it; also the membranes in the Sylvian fissure of the right side were rather thick, and the middle cerebral artery had a thick yellow patch on its wall, whilst the corresponding artery on the opposite side had none. On cutting off the vertex of the right brain in the usual way, a quantity of white liquid like milk came away, carrying with it some, and leaving more, of fine ragged shreds of white brain tissue, all soon becoming pink from admixture with a little blood. Curiously different was the state of the grey matter over the soft part; this was almost cheesy in consistency, and considerably yellow and brownish in patches; the softening extended nearly across the hinder half of the corpus callosum. On opening the ventricles the liquid therein was somewhat turbid, the lining not granular. There was no discovering the boundary of the ventricle at its hinder part, but there must have been one from the difference in the liquid in the soft brain from that in the ventricles.

In the left hemisphere were small soft patches, exactly similar in quality to the affection of the right; also in the corpus striatum were two deep hollows with yellow on their walls. The lining continued over them perfect, but sunk in a little; thin threads (vessels) crossed the cavities; the cerebellum appeared to be normal, also the optic thalami, the pons, and the medulla spinalis.

The microscope showed in the soft brain-substance granular cells in large numbers, and crowded numbers of small fatty cells in the yellowish firm cortical grey matter. I could not see any blood pigment in this. Many glistening bodies like in form to amyloid corpuscles were seen, but iodine made no change in them (colloid corpuscles).

Thorax.—The left *pleura* was slightly adherent behind. In the principal bronchus of the lower lobe of the right lung was a piece of apple impacted at the bifurcation. There was no tubercle of the *lung*. The lungs were flaccid, yet full of blood. The lower lobes contained little air, especially the lower right lobes, whose flotation was less considerable than that of other parts of the lung. The œsophageal epithelium was stripping.

The *heart* was very flaccid; the endocardium and valves much stained; no trace of vegetation nor ante-mortem coagula. The right heart contained a moderate amount of black clot. The left heart held a little liquid blood.

Abdomen.—The *alimentary canal* was normal.

The *liver* weighed 47 oz.; its substance was very flaccid from decomposition; no marks on the surface, and no syphilitic patches.

The *spleen* was flaccid; it weighed 4 oz.

The *kidneys* weighed 8½ oz., and appeared perfectly healthy.

The right *testicle* had a hard, almost gristly, opaque yellow mass embedded in its substance, and surrounded by fibrous formation, structurally continuous therewith; also a smaller hard mass in the epididymis. The left testicle was free from any nodules of the kind.

Had the new formation in the brain been the only one present, the characters of it were sufficiently pronounced to have led to the conclusion that it was of syphilitic origin. But a most significant feature of syphilitic formation is its appearance in different viscera of the same body. The same yellow cheesy matter is produced in the brain and testis—two organs as remote from each other in position and function as any two organs of the same body can be. Some common cause exists for the two identical formations. That this cause is not tubercular is shown in the absence of tubercle from its usual seats, especially from the lungs. On the other hand, the characters and position of the new matter correspond with those of ordinary

syphilitic matter, and the history of venereal disease, though it could not be traced in detail, gives some support to the conclusion drawn from the characters of the disease. In passing I may notice the absence of any increase of the temperature of the body, when so extensive a softening of the brain was present. This helps to show that white softening of the brain is not inflammatory. The thick yellowish node on the artery of the syphilitic part of the brain is of interest, and compares with that found in the case recorded by Dr. Hughlings Jackson ("Medical Times and Gazette," 1866). This state of vessel, together with the extensive syphilitic induration of the pia mater and grey matter, through which the white matter receives its vessels, will sufficiently account for the white softening of the brain.

II. Case in which the chief Disease was of the Respiratory Organs.

Case 8.—*Syphilitic disease of the throat (larynx), spleen, and liver; acute œdema of glottis.*—Amelia F., aged 20, admitted under Dr. Gull, Clinical Ward, 24th March, 1866; died 24th March, 1866.

A. F., had been attending for some weeks, as an out-patient, for syphilitic laryngitis, under Dr. Pavy, who, seeing her danger, sent her into Clinical Ward. She died on the same evening, in a sudden exacerbation of her dyspnœa, before assistance could reach her.

On inspection the body was well nourished. There was a scar just above the clavicle, in the lower triangle of the neck. No discoloration of surface.

Head.—Venous blood in great quantities in *brain* and membranes, but the anatomical state of the organ natural.

There was extreme œdema of the arytæna of the larynx; lower down the mucous and submucous tissues were swollen and hard, and on the left side, above the vocal cord, was the opening of an abscess, whose contents were semi-consistent, and were graduated into the thickened tissue around. This abscess rested on the arytænoid and cricoid cartilages, and the yellow semi-consistent matter extended under the perichondrium for a certain distance. The condition was that of gummous perichondritis.

Some of the cervical glands under the sternomastoid were enlarged, and almost pellucid.

Thorax.—In the upper lobe of the left *lung* a sub-pleural effusion of watery blood, about the size of a walnut, extended into the tissue, being continuous with lobular congestions in the

neighbourhood. The lung showed extreme congestion of the centres of the lobules.

The *heart* was small and firmly contracted.

Abdomen.—The *intestines* were healthy.

The *liver* weighed 48 oz. It was healthy as to substance generally. There was in right lobe a yellow nodule of size of a horse bean, and also a depressed patch one inch long, which was fibrous on its outer part, with an opaque yellow patch in it. This patch could be seen to contain wasting tissue charged with some fibre-growth.

The *spleen* showed a condition pronounced by me at first to be "phthisis of spleen," but with some misgiving. The sulphur-yellow masses, of the size of peas, softening at their centres, were utterly unlike anything I have ever seen in the spleen in cases of phthisis or tuberculosis; these were pretty uniform in size, and scattered plentifully deep in the organ. On seeing the syphilitic patch on the liver I had no doubt of their true character as syphilitic deposit in the spleen.

The *kidneys* weighed 7½ oz., and were quite healthy looking.

Syphilitic gumma of the spleen is not common. The formations present in the spleen in this case were markedly different from any other products that we are familiar with. They were almost all deep-seated, and differed thus from the "embolic" masses which we find in endocarditis. They were less white and less firm than the nodules present in "Hodgkin's disease," and, further, were not associated with enlargement of the lymphatic glands as in that disease. The lungs were entirely free from tubercles, and from this alone I should doubt exceedingly that these masses were tubercular. I have never seen anything approaching such an appearance in any of the tuberculous cases that I have examined. I have no doubt that this is an example of syphilitic gumma of the spleen.

III. Case in which Diarrhœa was the principal Symptom.

Case 16.—*Constitutional syphilis; waxy stomach and intestines, &c. &c.*—J. B., aged 24, formerly a soldier, and lately a labourer, was admitted into Stephen Ward, on July 7th, under Dr. Habershon.

He had syphilitic buboes six years ago, but had no chancre or discharge. He was received into Job Ward, under Mr. Poland, for fistula in ano, and sores on scrotum. He has had diarrhœa and vomiting since admission, and has passed blood to the amount of half a pint in his stools for two or three days; blood comes also from the mucous membrane of his mouth, and when he is asleep it collects in considerable quantity and coagulates. He has had pain in the long bones since admission, and also soreness of the tongue.

Present condition.—He has a large number of copper-coloured spots on forehead and cheeks, scaly circular spots of copper and rose colour on back, and very slightly on arms; and on the legs has had several larger sores, which have just ceased to discharge. He still vomits much of what he takes, though less than he did; there is no bile in the vomit. The tongue is furred and sore. Heart and lung-sounds normal. Pulse 84. Urine pale straw colour, albuminous. Sp. gr. 1023.

During his stay in the hospital he suffered from uncontrollable diarrhœa and vomiting, and grew very much emaciated before his death.

The post-mortem examination was made by Dr. Fagge. The *membranes* of the brain were opaque, and the *brain* itself anæmic. The *lungs* contained no ordinary tubercle; at one apex, besides a cavity there, a few masses which, when cut across, felt almost gritty. The *heart* was healthy. The mucous membrane of the stomach was pale, and markedly lardaceous. The *intestines* lardaceous in most marked degree. The *liver* was lardaceous, and apparently also fatty. The *spleen* lardaceous. The kidneys were small, weighing only seven and a half ounces together; they were most markedly lardaceous, iodine developing the characteristic dark colour in the pyramids as well as in the cortex.—*Guy's Hospital Reports, Vol. XIII*, 1868, *p.* 329.

86.—ON BUBO.

From a report in the LANCET on the practice at Guy's Hospital.

The following is an outline of the methods of treatment adopted by Mr. Durham in the ordinary forms of bubo.

1st. In the case of the numerous, slightly enlarged, hard painless glands usually associated with true syphilitic sores, no local treatment is considered necessary. The glands participate more or less directly in the general treatment. This essentially consists, in the majority of cases, in regular mercurial inunctions about the inguinal regions.

2nd. In the case of the less numerous (or single), more enlarged, painful and tender, but still somewhat hard, glands, associated with simple or suppurating venereal sores, or with syphilitic sores which have become inflamed, or sometimes with gonorrhœa, *rest*, as thorough as possible, is enjoined. At the same time, all practical measures are taken to remove the irritation and allay the inflammation of the parts primarily affected. When it can be borne, continuous pressure is maintained over the enlarged glands by means of a large pad of cotton-wool or lint, and a spica bandage. In some cases the iodide of potassium ointment, or the iodine ointment, is freely smeared over

the parts beneath the pad, and from time to time gently rubbed in. In other cases (especially in those in which pressure cannot be borne, and in which the patient is obliged, or is able, to lie up), the skin over the glands is repeatedly painted with the strong tincture of iodine, or with some blistering fluid. The former line of treatment is adopted in cases in which it is desired to act upon the glands by means of medicaments absorbed ; the latter in cases in wich counter-irritation seems likely to be useful, and in which it can be carried out without hurt or undue inconvenience to the patient. In many cases in which there has been much tenderness, a combination of extract of belladonna with iodide of potassium has been applied with apparent benefit. Every endeavour is made in every case to prevent suppuration altogether, or at any rate to put it off as long as possible, and to confine it within the narrowest limits. If, however, suppuration seem inevitable, warm fomentations and poultices are recommended.

3rd. In the case in which the skin is painful, red, and thinning, and in which it is manifest that suppuration has taken place in or about the glands, the buboes are at once opened by the knife in such a manner as to give the freest and quickest exit to the pus, and poultices are ordered to be applied for a few days. The direction and extent of the incision made vary with the shape, size, and condition of the abscess. As a rule, the incision corresponds in direction with the longest diameter of the abscess ; and the length of it is determined by the extent to which the thinned and damaged skin will probably be left "undermined" after the bulk of the matter has been discharged. A well-judged free incision in the first instance often prevents the necessity of subsequently slitting up troublesome sinuses, and of dividing portions of undermined skin which will not adhere to the subjacent structures. In cases in which the abscess is roundish in outline, the vertical incision is preferred. In the majority of instances, the abscesses are elongated in the direction of the inguinal fold ; in such cases the incision is made about parallel with Poupart's ligament.

In cases in which there is but a small, soft, fluctuating spot surrounded by more or less defined hardened structures, and in which the skin is but slightly reddened, or not at all, and in which there are few signs of acute inflammation, a small puncture into the abscess is considered sufficient.

In some cases the abscess appears to be in the structures surrounding the glands, rather than in the glands themselves. In such cases, after the matter has been evacuated, the glands may be seen standing out, as it were more or less enlarged and hard, and overlapped to some extent by the undermined edges of the incision. The application of strips of lint smeared with the red

oxide of mercury ointment to the glands is the treatment ordinarily adopted when the case has assumed a chronic aspect.

In cases in which open buboes take on a chancrous aspect they are treated on the same principles as the primary sores to which they correspond. Some are stimulated, as by nitric-acid lotion or black lotion; some are soothed by the application of preparation of opium or anodyne poultices. Occasionally, but rarely, destruction of the surface by strong nitric acid is necessary. In many instances the iron lotion (ferri potassio tartras, from ten to twenty grains in one ounce of water) is found useful, or a mixture of the iron lotion with some opiate.

In all cases the most thorough rest compatible with the patient's circumstances is insisted upon; and in cases in which general rest is impossible, local rest is ensured as far as practicable by means of pad and bandage. In all cases, also, the general health of the patient is carefully attended to, and any constitutional symptoms are treated from time to time as they may arise.—*Lancet, Nov.* 23, 1867, *p.* 641.

87.—TREATMENT OF SYPHILIS BY LARGE DOSES OF IODIDE OF POTASSIUM.

By Sir Henry Thompson, London.

For several years past, at least eight or ten, I have been in the habit of pointing out, at University College Hospital, to students and others, the value of large doses of iodide of potassium in certain forms or stages of syphilis. Commencing with twenty grains daily, I have frequently increased it to seventy-five. This fact has been, as is well known there, a favourite clinical topic with me, the curative effects of the drug having been often extremely remarkable. At the present moment a man in my ward is taking sixty grains daily; he has had seventy-five, and has continued that quantity for many days together.

After a very large experience of the remedy, I have arrived at the following conclusions:—

1st. That iodide of potassium in large doses (i.e., from thirty to seventy-five grains daily) is almost a specific for the cure of large spreading tertiary, or late secondary, ulcerations of the skin, such as those so common, and generally so intractable, affecting all parts of the body, and which often follow an eruption of rupia.

2nd. For those late syphilitic eruptions which affect the nose so disastrously, and often so rapidly, these same doses are equally valuable. I am in the habit of saying that "no man

ought to lose his nose from syphilis if he can take these doses."

3rd. Iodism is exceedingly rare in presence of tertiary syphilis; and large doses do not, as a rule, occasion loss of flesh. If they cure the syphilis, the patient usually gains weight under their influence.

I have the notes of numerous cases of large tertiary sores rapidly healed by this agent, and have often intended to bring the subject formally before the profession. But having given it constant publicity for so long a period st the hospital, I have thought this might suffice, and I am very glad that Dr. Pollock has done so. I am compelled to add that I consider iodide of potassium as undesirable in primary syphilis and in early secondary symptoms, as the iodide of mercury in *small* doses is valuable at that period of the malady.—*Lancet, Dec.* 28, 1867, *p.* 814.

88.—IODIDE OF POTASSIUM IN LARGE DOSES.

By Dr. R. S. Sisson, Maida-Hill.

During the years 1858-59, whilst a student at the Edinburgh University, I performed a series of experiments in order to test the effects of different drugs upon myself. In the Lancet of April 14th, 1860, you did me the honour of publishing the result of my observations on the iodide of potassium. To that report I beg to refer Dr. Julius Pollock for further details. Suffice it here to state that, with gradually increasing doses, I was enabled to take *one ounce per diem* of that drug with no other effects than slight catarrhal symptoms. I at that time thought that, in the treatment of syphilis, chronic rheumatism, &c., all the good effects of the drug might be obtained by small doses; but further experience has convinced me that large doses—twenty or even thirty grains—are not only well borne, but are frequently required in order to effect a cure.—*Lancet, Jan.* 18, 1868, *p.* 105.

89.—ON THE USE OF IODIDE OF POTASSIUM COMBINED WITH QUININE AND IRON IN THE TREATMENT OF SYPHILIS.

By Dr. Peter Eade, Norwich.

Like most other practitioners, I also have continually found the iodide, when given in small or moderate doses, fail to arrest the ravages of ulcerative secondary or of tertiary syphilis, but I have for some time past been aware of the singular increase to the efficacy of this drug which is produced by combining with it a salt of iron, or, what I find to be better still, a double salt of

iron and quinine. In this way I find that a dose not exceeding eight or ten grains of the iodide of potassium, with the same quantity of the citrate of iron and quinine, rarely fails to stay the progress of the disease, and has repeatedly, in my hands, been at once and progressively effective, when the same dose of the iodide by itself has exerted little or no power over its progress. Indeed, so frequently has this been the case, that my rule now is to begin at once with the compound mixture, instead of with the iodide alone, or in solution in the usual bitter infusion.

Iron has long been known to be very useful in tertiary and some secondary affections; and I doubt not that its action is to counteract and rectify the blood cachexia which is so markedly present in these cases. But I believe, also, that the quinine has its special advantage over other bitters; in all probability as more markedly opposing undue destruction of tissue. But whatever the theory of their action, I can state that my case-books now show many instances of most successful results from the above method, after the iodide alone, even in good doses, had been given in vain; and I have at this very time two patients under my care in the Norfolk and Norwich Hospital, suffering from severe tertiary affections, who are rapidly receiving the most marked benefit from this mode of treatment, of whom one—a woman—has suffered for nine years from ulceration of the nose, hard palate, and pharynx, and has been treated in vain at one—if not two—public hospitals, where it may be safely inferred she has taken the iodide of potassium in at least the usual doses.—*Lancet, Jan.* 18, 1868, *p.* 105.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

90.—ON TURNING IN MIDWIFERY.

By Dr. Robert Barnes.

If we were restricted to one operation in midwifery as our sole resource, I think the choice must fall upon turning. Probably no other operation is capable of extricating patient and practitioner from so many and so various difficulties. In almost every kind of difficult labour with a pelvis whose conjugate diameter exceeds three inches, it would be possible to deliver by turning with a reasonable prospect of safety to the mother, and in many instances with probable safety to the child. We might very greatly restrict craniotomy. We might dispense with the forceps; but neither forceps nor craniotomy will serve as a substitute for turning in special applications. It is difficult therefore to exaggerate the importance of carrying to the utmost limit the perfection of this operation. Yet the text-books exhibit a very inadequate appreciation of the subject. Turning by the feet was once said, not inaptly, to be the master-stroke of the obstetric practitioner. And still the operation was very imperfectly developed.

I propose to describe and illustrate with some fulness the conditions upon which mobility of the fœtus *in utero* depends, the various modes by which the fœtus may be made to change its position, and the application of this knowledge to the practice of turning, embodying the teaching of Wigand, d'Outrepont, Radford, Simpson, d'Esterlé, Lazzati, Braxton Hicks, myself, and others.

Having regard to the various allied operations which it is convenient to class under a general description, I would define *Turning as including all those proceedings by which the position of the child is changed in order to produce one more favourable to delivery.*

There are three things which it is very desirable to know as much about as possible before proceeding to the study of turning as an obstetric operation—

1. What are the conditions which determine the normal position of the fœtus *in utero?*

2. What are the conditions which produce the frequent changes from the ordinary position?

3. What are the powers of nature, or rather the methods employed by nature, in dealing with unfavourable positions of the fœtus?

1.—*The Conditions that determine the Normal Position of the Fœtus in Utero.*—It would be idle to do more than glance at the fanciful ideas upon this subject that have obtained currency at various times, although most have an element of truth in them. Ambroise Paré believed that the head presented owing to the efforts made by the child to escape from the uterus. Even Harvey believed that the fœtus made its way into the world by its own independent exertions. Dubois endeavoured in a long argument to show that the fœtus has *instinctive power*, which determines it to take the head-position. Simpson rightly concluding that the maintenance of normal position depends very much upon the life of the fœtus, observes that it has no power of motion except muscular motion, and infers that the fœtus adapts itself to the uterus by *reflex muscular movements* excited by impressions—as by contact with the uterus—upon its surface. Thus we come down by a curious scale of theories, in which the philosopher may trace the influence of contemporary physiological doctrines or knowledge. First, the fœtus is endowed with the high faculty of *volition;* then it falls to the lower faculty of *instinct;* and lastly, it is degraded to the lowest nervous function—that of *reflex* motion. I should be disposed to estimate at a still lower point the influence of the fœtus as an active agent in maintaining its position during pregnancy or labour. It is incontrovertibly true that the normal position of the fœtus and the course of labour are intimately dependent upon the life of the fœtus. But I think I am enabled to affirm from very close observation that a fœtus, if full grown and *only recently dead*—that is, for a few hours—may be nearly as well able to maintain its position and to conduce to a healthy labour as one that is alive. How is this? It depends simply upon the preservation of sufficient tone and resiliency in the spinal column and limbs to maintain the form and posture of the fœtus. Whilst alive, or only recently dead, the spine is firmly supported in a slight curve, the limbs are flexed upon the trunk, the whole fœtus is packed into the shape of an egg, which is very nearly the shape of the cavity of the uterus. It has a long axis, represented by its spine. This long axis, being endowed with sufficient solidity, resembles a rod, rigid or only slightly elastic. It is a lever. Touched at either pole, the force is propagated to the opposite pole. If the head impinge upon one side of the uterus, the breech will be driven into contact with the

opposite point of the uterus; head and breech will move simultaneously in opposite directions. In labour, when the uterus is open to admit of the passage of the fœtus, the propelling power applied to the breech is propagated throughout the entire length of the spine or long axis, so that the head, the end furthest from the direct force, is pushed along in the direction of least resistance, turning at those points where it receives the guiding impact of the walls of the canal.

When the fœtus has been some time dead, the elasticity and firmness of its spine are lost; flaccidity succeeds to tonicity. Force applied to one extremity is not propagated to the other extremity—or, at least, very imperfectly so; the long axis bends, doubles up like a rod of gutta-percha softened by heat. If, the fœtus in utero being in this state, pressure be applied to one side of the head, the head will simply move towards the opposite side of the uterus. And if labour be in progress, the propelling force applied to the breech will not be duly transmitted to the head, but will tend to double up the trunk, to make it settle down in a squash in the lower segment of the uterus or in the pelvis. The head—the cervical spine having lost its resiliency—will not take the rotation and extension turns. It will run into the pelvis like jelly into a mould. Or, at an earlier stage, the limbs, especially the arms, having lost their tonicity, drop or roll in any direction under the influence of gravity or of pressure; and hence may fall into the brim of the pelvis, constituting what are called transverse presentations. The influence of this law is clearly seen in the course of that process called "spontaneous expulsion," by which a dead child is expelled, a shoulder presenting.

Other factors besides the child have to be considered. Scanzoni correctly observes that the frequency of head presentation is dependent on the operation of various causes. 1. There is the force of gravitation; 2. The form of the uterine cavity; 3. The form of the fœtus (to which must be added the properties I have described due to life or death); 4. The quantity of amniotic fluid; 5. The contractions of the uterus during pregnancy and the first stage of labour. In the early stages of pregnancy the embryo is so small relatively to the cavity containing it that it floats suspended in the liquor amnii. But about the middle of pregnancy the fœtus grows rapidly; it acquires form; and, at the same time, the uterus grows more in its longitudinal than in its transverse diameter. As soon, therefore, as the fœtus—an ovoid body—attains a size that approaches that of the capacity of the uterus, the walls of the uterus will impose upon the fœtus a vertical position. The fœtus has become too long to find room for its long diameter in the trans-

verse diameter of the uterus. Mutual adaptation requires that the long diameters of fœtus and uterus should coincide.

A condition not, to my knowledge, hitherto noticed, which has a powerful influence upon the determination of the child's position in utero, is the normal flattening of the uterus in the antero-posterior direction. In the non-pregnant uterus, the cavity of the body—the true and only gestation-cavity—is a flat triangular space, the angles of which are the orifices of the Fallopian tubes and the os internum uteri. A similar triangular superficies is marked out on each half of the uterus, anterior and posterior. The anterior superficies lies flat against the posterior superficies, touching it as if the two were squeezed together. When pregnancy supervenes these surfaces are necessarily separated to form a cavity for the growth of the ovum. But the original form is never entirely lost. The cavity is always more contracted from before backwards than from side to side. This is proved by direct observation if the fingers are introduced after abortion or the hand after labour at term. The uterine cavity is closed by the flattening of the anterior and posterior walls together. This takes place the moment the uterus contracts. If the finger or hand be in the uterus at the time, this is plainly felt. Now, this flattened form of the uterus is the reason why the fœtus takes a position with either its back or its belly directed forwards. The fœtus is broader across the shoulders than from back to front, and therefore its transverse diameter is fitted to the transverse diameter of the uterus. There is a physiological design that dictates the downward position of the head. The fundus is the part designed for the implantation of the placenta, where it can grow undisturbed, and continue its functions during the expulsion of the child. The lower part of the cavity is therefore left free for the development of the embryo. Why the back is commonly directed forwards to the mother's belly is this—The child's back is firm and convex; its head is also firm and convex behind. The anterior aspect of the child's body is plastic and concave, and therefore fits itself better to the firm convexity of the mother's spine. It is clear that the two solid convex spines of mother and child would naturally repel each other; and the child being movable, it is the child's back that recedes, turning forwards.

2.—*The conditions which produce the frequent Changes in the Child's Position.*—Any considerable disturbance of the correlation of the factors which keep the fœtus in its due position of course favours malposition. The principal disturbing conditions may be stated as follows:—*An excess of liquor amnii* acts in two ways—first, it favours increased mobility of the fœtus; secondly, it tends to destroy the elliptical form of the uterus. The transverse diameter increasing in greater proportion than the longi-

tudinal, the cavity becomes rounder. Hence the fœtus is no longer kept in a vertical position for want of the proper relation between its form and size and those of the uterus.

Obliquity of the uterus was considered by Deventer to be a main cause of malposition. It is now very much discredited, but I am disposed to believe that it has, not seldom, a real influence. Dubois and Pajot showed that in 100 women 76 exhibited a marked lateral obliquity to the right, four to the left, and twenty an anterior obliquity. Wigand had shown that deviations of the uterus to the right and forwards were far the most frequent. The normal direction of the non-pregnant uterus is nearly that of the axis of the pelvic brim. As it grows during pregnancy, rising above the brim, the projecting sacro-vertebral angle and the curve of the lumbar column deflect its fundus to one or other side; and, if the abdominal walls be very thin and flaccid, the fundus will fall forwards. The tendency of these obliquities, if carried beyond ordinary measure, is to throw the axis of the uterus out of the the axis of the pelvic brim, and to bring some other part than the vertex of the fœtus to present. The probability of this will be increased by the irregular contractions of the uterus likely to be excited by parts of the fœtus pressing unequally upon its walls. For example, in extreme lateral obliquity the breech may press strongly upon one side of the fundus; contraction taking place here, will drive the head further off the brim on to the edge, where, if it finds a *point d'appui*, it will rotate on its transverse axis, producing forehead or face presentation, and favouring the descent of the shoulder. Wigand explains how a too loose and shifting relation of the uterus to the pelvis disposes to cross-birth. In this condition it is observed that the head is now fixed in one place, now in another, and now not felt at all.

He further says that any obliquity of the uterus exceeding an angle of 25° is unfavourable; and that even a lesser obliquity, with excess of liquor amnii or a small child, is likely to cause the presenting head to be displaced, and to bring a shoulder into the brim, especially if strong pains or bearing-down efforts be made *early* in labour.

He explained that the os uteri might be brought over the centre of the brim by internal drawing upon the os, combined with external pressure upon the fundus in the opposite direction, thus putting in practice the principle of acting simultaneously upon the two poles of the uterus.

Deformity of the pelvis or lumbar vertebræ is often a powerful factor. The comparative frequency of transverse presentations in cases of deformed pelvis is certainly greater than where the pelvis is well formed. I think, however, that *slight* deformity

has more influence in causing malposition than extreme degrees. In these latter, malpositions are rarely observed.

The attachment of the placenta to the lower segment of the uterus is, as Levret has clearly shown, a cause of malposition by forming a cushion or inclined plane, which tends to throw the fœtal head out of the pelvic axis across the brim. Hence the frequency of cross-birth and of funis-presentation in cases of partial placenta prævia. But there are numerous cases in which the placenta dips into the cervical zone, growing downwards from the posterior and lateral walls of the uterus, without leading to hemorrhage, and thus not suspected to be cases of placenta prævia, which, nevertheless, form an inclined plane behind or on one side, and produce malposition.

Then there is the *influence of external forces*, as of pressure applied to the uterus through the abdominal walls. The dress of a woman at the end of pregnancy is a matter of no small moment. The pressure of a rigid busk of wood or steel upon the fundus of the uterus, modified by the various movements and postures of the body, may flatten in the fundus, thus reducing the longitudinal diameter of the uterus, or it will push the fundus to one side, causing obliquity. It will, at the same time, press directly upon the breech, and thus tend to give the fœtus an oblique position, throwing the head out of the pelvic axis. Pluriparæ should do the reverse of this. They should wear an abdominal belt, which supports the fundus of the uterus from below upwards.

Want of tone in the uterus, which implies inability to preserve its elliptical form, and a tendency to fall into rotundity, a form which obviously favours malposition.

Irregular or partial contraction of the uterus cause malposition. Naegele insisted upon this. He found that in some cases malposition was averted by allaying spasm.

The researches conducted by several German physicians, amongst whom I may cite Credé, Hecker, and Valenta, establish the fact that the fœtus changes its position with remarkable frequency. Valenta examined 363 multiparæ and 325 primiparæ in the latter months of pregnancy. He found that a change of position took place in 42 per cent. Change was more frequent in multiparæ, and in these in proportion to the number of previous pregnancies. Narrow pelves very frequently cause change of position. Circumvolutions of the cord, so often observed, are produced by changes of position, and hence bear evidence to the correctness of the proposition. It is interesting to observe that the general tendency of changes of position is towards those which are most propitious. Thus, cranial positions are least liable to change. Oblique positions are especially liable to change. These mostly pass into the long axis by spontaneous

evolution. *Self-evolution is a very frequent resort of nature.* In some cases several changes of position have been observed in the same patient. The presentations are made out by external manipulations. Valenta thus describes his method of ascertaining a breech-position during pregnancy:—He lays his right hand flat on the fundus uteri, and then strikes the tips of the fingers as suddenly as possible towards the cavity of the uterus, against the part of the child lying at the fundus. By this manœuvre he has always succeeded in recognising the head, if lying at the fundus, by its peculiar hardness and evenness. He detects the head in oblique and cross positions in the same manner. P. Müller relates a case in which within five days a complete version of the fœtus was effected six times.

Yet the fact of the "spontaneous evolution" of a living child, as described by Denman from actual observation, has been doubted!—*Medical Times and Gazette, Dec.* 21, 1867, *p.* 667.

91.—ON THE USE OF THE VECTIS IN ASSISTING PARTURITION.

By Dr. G. D. R. McCarthy, Esq., Wellington, Salop.

[Mr. McCarthy for many years used the forceps, but now for some years has greatly preferred the vectis. He says:]

I have found the vectis the best instrument. For if, after all, it cannot extract the head when it has descended into the lower region of the pelvis, the forceps can but be used, and would require but little trouble to apply them. It has nearly superseded the use of the forceps in my practice. Not entirely; for occasionally one meets with a case of impaction which calls for the forceps or even the perforator; though I am thankful to say I have not met with many such. I consider it as a sort of artificial hand. An accoucheur must often wish he could introduce his hand so as to pass it behind the head and help it in its passage. Now this desideratum is in a great measure potentially, though not actually, secured by the vectis in the manner in which I have used it.

My method is to introduce the four fingers of my left hand, well anointed with lard, with their backs to the perineum, and, bringing the handle of the instrument to the pubes, the thighs being held open, slide the convexity of the curve along the inside of my fingers and then between the sacrum and the head, which is often done in a minute or two. I then bring the handle to a line in the direction of the upper axis of the pelvis, and, first depressing it—the patient lying at the edge of the bed—and then gently elevating it, the curved part passes over the head. Then, holding the handle steadily with my left hand, and care-

fully maintaining a sustained traction, examining with my right the progress, I keep the head from receding in the interval of the pains, which, as a proof how glad—if I may so express it—Nature seems of seasonable assistance, become more powerful and effectual. If I do not gain anything, after a short time, I partially withdraw the instrument and introduce it on the lower side of the head, when it lifts it up like a lever, using some degree of traction at the same time. If I find I cannot slide the instrument over the head as I wish, I withdraw it for a time and then introduce it again, when it generally succeeds. Under these circumstances, I give the ergot in decoction, and thus gain all the natural and artificial combined power possible.

I am happy to say that I have greatly facilitated and accelerated the delivery of many by this simple instrument, using it, as I have said, like a hand, and never injured the mother, and seldom the child. I believe it requires a little tact, which may be gained by practice; and whoever has tried it somewhat frequently—I mean as often as the protracted nature of cases, or the failing of natural efforts, have caused alarm and anxiety to any degree—has, I dare say, experienced its usefulness.—*British Medical Journal*, *Dec.* 14, 1867, *p.* 542.

92.—ON THE INDUCTION OF PREMATURE LABOUR BY INJECTION TO THE FUNDUS OF THE UTERUS.

By Prof. J. Lazarewitch, Professor of Midwifery at the University of Kharkoff, Russia.

[The safest, most effectual, and readiest method of inducing premature labour is undoubtedly that known as Cohen's, viz., the injection of water by means of a catheter between the uterine walls and the fœtal membranes. After preliminary remarks to the above effect the author recapitulates as follows:]

1. The surest methods of induction of premature labour are those in which the body or fundus of the uterus is excited.

2. In the method of Cohen, the greater the quantity of fluid used in injections the nearer it reached to the fundus of the uterus, the surer was the action.

3. When, after injection, the fluid passed upwards to the fundus of the uterus, violent contraction always ensued immediately.

4. In the experiments on rabbits the thickest part of the uterine walls proved to be the most irritable; in a pregnant woman the body and fundus of the uterus is the thickest.

5. On introducing the sound into the cavity of the uterus in its normal state, pain is caused only by contact with the fundus.

6. At the time of parturition contractions of the uterus are more quickly excited by rubbing the fundus of it than by the irritation of its inferior section.

7. The inferior section of the uterus is exposed to many different irritations, and therefore its sensitiveness is blunted.

8. The commencement of parturition is caused by the gradual separation of the membranes of the ovum from the walls of the uterus, and when this separation is complete, nearer to the fundus of it.

From the above statements we may conclude that, *for sure action of injection, it is necessary that the fluid injected should approach as near as possible to the fundus of the uterus*, this being the most sensitive to irritations.

The fluid injected by my method is directed upwards to the fundus of the uterus, there spreading, separates from it the membranes of the ovum, and flows back only after some minutes, or even after some hours, because it is easily retained in the superior section of the uterus. In this manner considerable and rather protracted irritation is produced at the fundus of the uterus.

In those cases of induction of premature labour in which the fluid was not retained in the cavity of the uterus, but flowed back immediately after injection, there was little or no effect, because the fluid spread only in the inferior part of the uterus, little sensitive to irritations. Cohen was convinced that for a successful injection a conical tube should be employed, which, being closely applied to the orifice of the uterus, would thus prevent the fluid from flowing back. In my opinion the fluid should, in this case, be directed to the fundus of the uterus. But if injection is used, when a woman is brought to bed not for the first time, in the last month of pregnancy, when the vaginal portion is very soft, and its internal and external orifices are wide open, it is impossible to press the tube to the orifice of the uterus, so as to prevent the fluid from running back; still less possible is it when the uterine orifice is considerably distended, and it is necessary to excite lingering labour; in these cases, as in all others, it is very convenient to use also a conical tube, not with the end closed and side apertures, as Cohen advises, but with a single aperture at the end, in such a case all the fluid injected must pass in an upward direction, to the fundus of the uterus. In this modification of the tube the difference between my method and that of Cohen consists—by his method the injected fluid must pass into cavity of the uterus, by mine it must be directed *to its fundus*.

The tube employed by me in the first three cases, which I am about to describe, is elastic, from six to eight inches in length, one end being one and a half line in diameter, and the other

six lines; its small end is somewhat rounded, and has one aperture. For the more convenient introduction of the tube, and to give it the necessary curvature, a metallic wire is introduced into it, bent to a right angle in two opposite directions. Thus a joint is formed, which retained by the edge of the larger aperture of the tube, does not admit of its passing through the narrow aperture of the tube. The tube is fixed to the end of a syringe, and is adjusted to it by means of gutta percha.

In 1861, during my stay in Vienna, the instrument maker Leiter adjusted the above-described tube to an india-rubber bag, which worked like a syringe. With such an apparatus Professor C. Braun in one of the following cases employed my method of induction of premature labour.

In the same year, at Berlin, Professor Martin made to me the following remark, respecting my method of injection: he considered the possibility of air passing into the cavity of the uterus to be dangerous.

It becomes quite impossible with a well constructed apparatus for injection and the proper employment of it. After *filling the syringe, and holding it with the handle downwards, the piston should be moved till the air, rising to the end of the tube, be expelled by the water, which has to pass to the aperture of the tube.* The piston must move easily, and fit the cylinder exactly.

For the purpose of injecting fluid into the cavity of the uterus, M. Edelberg in Kharkoff has, after my direction, constructed a special apparatus. This apparatus consists of a metallic cylinder seven and a half inches in diameter. A piston furnished with an indented stem is worked by a toothed wheel, with a spring inclosed in a drum attached to the upper part of the cylinder. The spring is wound up by a small crank, which may be taken off during the operation. The action of the spring, and consequently of injection, may be retained with a stop. By slightly pressing a knob it is easy to connect the stop with the toothed wheel, and in this manner arrest the action of the spring. On the smooth side of the stem are divisions marking the quantity of fluid contained below the piston.

To the cylinder a tube is attached six inches long, and three lines thick, with one aperture at the end of it. This tube is made of pliant metal, and therefore it may be bent at pleasure; on its concave side it is marked to show how much of it is introduced into the cavity of the uterus.

Another tube, similar to that described above, but with a closed end, and with two apertures, one in front and the other behind. The fluid injected through this tube must spread over the uterus, and cannot take a side direction to the orifice of the viaduct.—*Obstetrical Transactions, Vol. IX, p.* 167.

93.—ON THE INDUCTION OF PREMATURE LABOUR BY THE DOUCHE.

By Dr. Thomas Telford, Ex-Assistant Physician, Rotundo Hospital, Dublin.

The induction of premature labour in cases of undersized pelvis, is an operation of great importance to the practitioners of midwifery, and has, for a long time, engaged their attention; by its means we are sometimes enabled to save the life of the child, and thus add greatly to the happiness of the parents. Various modes of proceeding have from time to time been adopted, and with a varying amount of success—such as rupturing the membranes, separating the membranes round the os, passing a catheter between the membranes and the wall of the uterus, by the hydrostatic dilator of Dr. Barnes, the administration of ergot, and by the douche; the latter is the plan which I adopted in the three following cases, in preference to any other.

Rupturing the membranes is very uncertain in its action. I have frequently seen women come into hospital hardly in labour, though the membranes had been ruptured some days before admission. Early rupture of the membranes predisposes to a rigid condition of the soft parts, adding a new difficulty to the case; the separating the membranes, or passing a catheter between the membranes and uterus, unless done with great care, is likely to rupture the bag of waters, and is, therefore, open to the same objection. Of Dr. Barnes's dilator I have no experience. The administration of ergot, I am confident, is not efficacious in producing labour, as, from a series of experiments I made under the direction of Dr. Denham, master of the hospital, for the purpose of testing the toxic effects of ergot on the fœtus, we found that ergot administered to a woman seven or eight months pregnant, neither induced labour, or exerted any deleterious effect on the child.

The application of the douche is extremely simple, it is best done by placing the patient in the usual obstetric position, with the hips drawn well over the edge of the bed; by passing a full-sized Ferguson's speculum, the os is brought into view, the nozzle of an ordinary syphon syringe is then inserted into the os, and a continuous stream of water injected into the cavity of the uterus. On the withdrawal of the syringe, the water pours out of the uterus in a full stream; this may again be repeated a couple of times; one such application will generally induce labour within twenty-four hours. Before using the syringe you must be careful to fill it with water, so as to exclude the admission of any air into the uterine sinuses. Some physicians advise the alternate use of hot and cold water; this, I think, a matter

of unimportance; tepid water is most agreeable to the patient, and should therefore be used; otherwise, as the action of the water is altogether mechanical, it is immaterial whether we use hot or cold. The action of the water closely resembles the action of the uterus, by gradually separating the membranes around the os and cervix uteri. The three following cases illustrate the rapidity with which labour is induced.

Mary Brien, aged 23, pregnant of her second child, was admitted to hospital, November 28th, 1866. She had been delivered in the Rotundo Hospital, at Christmas, 1865, by the crotchet, owing to narrowing in the antero posterior diameter of the brim. She was then advised, should she again prove pregnant, to come into hospital between the 7th and 8th month, in order that premature labour might be induced, as we feared, owing to the narrowing which existed, a child at the full term could not be born alive. She neglected to come into hospital till just eight months pregnant, when the probability of saving the child was much less than had she been seen earlier. Having cleared out the bowels, I douched her in the manner already described at 12 noon, November 30th; labour commenced at 2 p.m., the pains being short and frequent. At 12.30 a.m. of December 1st, the membranes ruptured, no presentation being then discernible, it being altogether out of reach of the finger.

At 8.30 a.m., I was called to see her, and, on examination, found the right hand in the vagina; the child was alive, as proved by the hand grasping the finger, when introduced. Version was at once determined on, and chloroform having been administered, I passed my right hand, and turned with the greatest care; the breach and body of the child were then delivered; the arms offered considerable resistance, and were with much difficulty brought down; the delivery of the head was then attempted in the usual manner, and a considerable amount of extractive force used by myself and Dr. Denham, but without avail. Fearing that any further force might tear the vagina from the neck of the uterus, I delivered the child by perforating behind the ear and using the crotchet; the child was a male, and much larger than most eight months' children. Her convalescence was rather slow; she suffered from some tenderness of the abdomen for a few days, and afterwards from an attack of sciatica, but went out quite well in a couple of weeks.

The second case is that of Eliza Scott, aged 26, pregnant of her fourth child. Her first two children were delivered by the crotchet. She was then advised to come into hospital when seven months pregnant, should she again prove so; this she did, premature labour was induced by the douche, and she was

delivered of a live child in Sept. 1862 ; the child lived till eleven months old, when it was carried off by measles. Finding herself again pregnant, she applied for admission on the 28th of February of the present year, being at the time nearly eight months gone. The bowels having been opened, the douche was applied on March the 1st, at 11 a.m., and again at 4 p.m. ; labour came on that evening, the membranes ruptured at 11 a.m., March 2nd, the os being then fully dilated. The pains were quick and strong, but as the head had made no advance at the end of two hours, and the fœtal heart increased in rapidity, delivery by the forceps was decided on. With the assistance of Dr. Cronyn she was delivered, after considerable difficulty, of a healthy male child. Her convalescence was most satisfactory, not a single bad symptom having arisen.

The third case is that of Mary Doyle, aged 21, pregnant of her second child. She had been delivered by the crotchet June 25th, 1866, and was then advised to come in when seven months pregnant, this she neglected doing till past the eighth month. The douche was applied at 11 a.m., June 21st, 1867 ; labour came on almost immediately ; the membranes ruptured at 5.30 p.m., when the right arm was found in vagina. She was put under chloroform and version performed ; the arms were brought down with ease, but no amount of traction would deliver the head ; the head was then perforated through the mouth and extracted ; her convalescence was most satisfactory, she going out on the eighth day.

The foregoing cases are interesting as showing the certainty with which premature labour can be induced by means of the douche, when properly employed. In the Medical Times and Gazette for November 2nd of the present year, a paper was read by Professor Lazarewitch, "On the Induction of Premature Labour by Injection to the Fundus of the Uterus." This operation, however, requires a special apparatus, and is not more effectual than the douche used in the ordinary way. [See preceding article, 92.]

In two of the cases related the arm was the presenting part, and turning had to be performed. This operation is recommended by Sir James Simpson in cases of undersized pelvis, where the head is the presenting part. On the supposition that the head will mould itself to the pelvis, I have tried it myself in three cases, and seen it done once or twice by others, but the result did not equal my expectations. Of three cases I had myself, the child had to be delivered by the crotchet in all, and in one the uterus was torn from the vagina, the woman dying in half an hour ; yet, those were cases considered favourable for the trial.

These cases are brought forward in order to show the facility with which premature labour can be induced.—*Medical Press and Circular, Nov.* 27, 1867, *p.* 483.

94.—ON A CASE OF PUERPERAL FEVER, OR PUERPERAL PYÆMIA, AFTER AN ABORTION.

By Dr. Snow Beck.

Paper read before the Obstetrical Society of London.

In this case, abortion was induced at the end of the fourth month, in a healthy young lady, aged twenty-six. After a short time the symptoms observed were, dirty and muddy complexion, great weakness, intense thirst, constant retching, cold perspirations, extremely rapid pulse, mind clear, though wandering sometimes, and much irritability. She died on the sixth day after the abortion. The post-mortem examination showed the uterus large and flabby, a portion of placenta imbued with purulent fluid, adherent to the uterus, and attached to it a considerable amount of coagulated blood; purulent fluid in the uterine sinuses, which were otherwise healthy, but so patent as to admit of fluid being injected through them; peritoneum not injected, though the pelvic portion was covered by a thin layer of soft lymph, and there was effusion of brownish serosity into the cavity; lobular congestion of the lungs; effusion of serum into the pleuræ, with soft lymph on the surface; small collections of similar exudation beneath the pleura. The minute structure of the internal surface of the uterus was fully detailed, and the relations of the placenta found to accord with the descriptions of the Hunters and others. The case appeared clearly to show the purulent infection of the general system through the pervious state of the uterine sinuses; also, that one of the most fatal forms of puerperal fever arose from this cause; the sinuses being pervious in consequence of a want of permanent contraction of the uterus. The statement of Dr. Barnes that he had repeatedly seen puerperal fever after perfect contraction of the uterus, was examined, and the cases related by him were considered by the author to be diseases very different from puerperal fever,—viz., Bright's disease, obstruction of the gall-duct, acute atrophy of the liver, scarlatina, and phlegmasia dolens. Lying-in women, when exposed to the infection of zymotic diseases, frequently took those diseases, but their essential characters remained unchanged. They often existed epidemically, and were infectious, and hence arose the assumed epidemic character and infectious nature of puerperal fever. In the treatment it was considered of much importance to prevent the purulent infection, by effecting a firm and per-

manent contraction of the uterus; when it had occurred, to cleanse out the uterine cavity, and give the sulphites. The large administration of brandy, and the application of a light bandage were prejudicial, and the latter impossible, from the tenderness of the abdomen and uterus.

After some remarks from Dr. TYLER SMITH, the following letter from Dr. BARNES, who was unavoidably absent, was read:—"I am confident that contraction of the uterus, although a very desirable object to attain, is not a security against puerperal fever. I wish also to observe that Dr. Beck bases his criticisms upon five cases referred to in my lectures, as if these cases embodied very complete views upon puerperal fever. The lectures were broken off for want of time. I had, of course, a great deal to say about septicæmic or pyæmic puerperal fever arising in the patient's own system. I maintain that the division I propose in those lectures on puerperal fever into excretory, uræmic, cholæmic, scarlatinal, septicæmic, &c., is of great clinical value. For example, a lying-in woman is taken with fever. It is very difficult, sometimes impossible, at first, to tell whether that fever is really of epidemic origin, as scarlatinal, or whether it be strictly of puerperal origin. The puerperal state will impress its stamp upon any kind of fever, no matter whence obtained. Therefore it is a sound clinical proceeding to regard every fever in a lying-in woman as puerperal fever first; then to endeavour to analyse the symptoms with a view to discover the peculiar or special nature of that fever. To say that true puerperal fever is not contagious is to refuse to believe in one of the best-attested facts in medicine."

Dr. PLAYFAIR said that he was in the position to relate a fact connected with puerperal fever which seemed to him to be of itself sufficient to negative Dr. Beck's theory that zymotic diseases were not modified by the puerperal state. Some years ago a lying-in ward had been established at King's College Hospital. The utmost care had been taken in the construction and management of the ward, but in spite of every precaution the mortality had been for the last year or two excessively high. About a month ago numerous cases of erysipelas appeared in the surgical wards, and immediately afterwards the two most recently confined women were attacked with a very adynamic form of puerperal fever, which proved fatal in both instances. There can be no doubt that the cause of the disease was the same as that which was producing erysipelas in the surgical wards. There was, however, no trace of erysipelas as such in the puerperal cases, and the natural inference was that the action of the poison was modified by the state of the patients. He was happy to state that the authorities of the hospital had now determined to close the ward altogether. Doubtless a re-

laxed condition of the uterus was, as Dr. Beck maintained, a strong predisposing cause of puerperal fever; but he believed it would be a most false and dangerous conclusion if we were to generalise from this fact, and overlook the other and well-established modes by which puerperal fever was produced.

Dr. Graily Hewitt stated, that the views he had on a former occasion enunciated on the value of pressure by means of the binder in the early stage of puerperal fever, and as to the extreme value of large doses of alcoholic stimulants at the same period, he still maintained. In a large number of cases, an account of which he hoped shortly to bring before the Society, he had tested their value.

Dr. Wynn Williams considered the disease in question to be identical with that met with during the progress of other diseases, but objected to the name pyæmia, believing it to be an accidental occurrence during the progress of a certain disease, and not always present. He alluded to septicæmia, which was caused by the absorption into the system of the putrid emanations from decomposing animal matter. Pure pus itself, when introduced into the circulation, was no more injurious than any other foreign material, as quicksilver. When scarlatina, erysipelas, and other allied diseases are present, the air is always contaminated with putrid emanations, and this, being brought in contact by vibrios generated in such an atmosphere, or other ways, readily sets up putrefaction in the discharges of the parturient woman, and these, being retained, are very apt to infect the patient. If, therefore, in the discharges there is the least offensive smell, it is right at once to have recourse to some disinfectant injection, of which a solution of iodine is the best.

Dr. Braxton Hicks was inclined to join issue with Dr. Beck upon the statement that this preparation tended to prove the existence of open mouths into the placental cavity. He held it almost certain that such openings did not normally exist. In fact, Hunter had overlooked the existence of the delicate membrane which represented the wall of the vessel as it passed through the innermost layer of uterine tissue to ramify on the mucous surface opposed to the placenta. The injection Hunter employed burst through this membrane, and thus it appeared to lead into the placental cavity. This membrane would readily be destroyed under the conditions in which the uterus was placed in the case related. But he thought we should make a dangerous and retrograde step if we cease to acknowledge the zymotic diseases as a cause of so-called puerperal fever. He considered that that disease may be divided into two great classes, with a few minor kinds—namely, that produced by decomposition of a clot, or the secretions in the uterus, thence

called endogenetic; and that derived from zymotic poisons, of which scarlatina and erysipelas were the most common. He had in his own practice found that about three-fourths of the puerperal fever he saw was somehow mixed up with scarlatina. But in the lesser class, owing to the decomposition of clots, he thought very great benefit was to be derived from washing out the uterus in all cases where the discharges were offensive. Of disinfecting fluids he preferred the solution of permanganate of potass, because it showed when it had done its work. It was a curious fact that offensive lochia had been present in all cases but one of acute puerperal mania which he had seen.

Dr. Brunton said that, with regard to the theory of the production of puerperal fever by zymotic poison, his experience was decidedly negative. He had attended several confinements in which, not only was there scarlet fever in the house, but the children were actually lying ill in the room, and no bad symptom had occurred to the mother during her convalescence. He agreed with Dr. Beck to a considerable extent, especially as to the advantage of securing a firm contraction of the uterus after delivery.

Dr. Sansom could not agree with Dr. Tyler Smith, that drugs were hopeless when once pyæmia had set in, and that "antiseptics were the feeblest of all feeble measures." He was glad to hear that the author of the paper had found the treatment by the sulphites successful. He believed Dr. Polli's introduction of the sulphites into therapeutics to be one of the most valuable additions to medical science. He did not agree, however, with Dr. Polli in his idea that the sulphites do not act upon the organised germs of disease, but upon the pabulum in which these germs are propagated. He believed that they acted directly as septicides. Dr. Sansom stated that he had succeeded in producing a series of salts which combined the diffusibility of the sulphites with the potential efficacy of carbolic acid. These agents, the sulpho-carbolates, he would take an early opportunity of introducing to the notice of the profession.

The President agreed with the author in his view of the pathology of the case which he had brought before the Society. It was one of puerperal septicæmia, due to the absorption into the blood of putrid matter from the uterus. But while admitting that perfect contraction of the uterus was most important, he could not consider the want of it as more than an occasional element in producing puerperal fever, which had many other sources. He was of opinion that cases of sporadic peritonitis, should not be tabulated with cases of puerperal fever, as the former arose from ordinary causes of inflammation, while puerperal fever, whether accompanied or not by peritonitis, arose from the introduction of poison into the blood,

either by the veins or the uterus, or by the lungs in inspiration. Contrary to the author, he was a strict believer in the contagion of puerperal fever immediately from patient to patient, and medially through a third person. He was also convinced that other fevers, as typhus, scarlet fever, measles, and the emanations from hospital gangrene, had been prolific causes of puerperal fever. The taint conveyed from post-mortem examinations was another source of puerperal fever. This was well illustrated by the statistics of the Vienna Lying-in Hospital, where at one time, when the students passed directly from their dissections to the lying-in women, the mortality was 1 in every 10 deliveries. When this arrangement was changed, the per-centage of deaths fell to 1 in 74. Lying-in hospitals are no doubt a boon to poor women without homes, but one greatly mitigated by the fact that the mortality in them from all causes is rarely less that 1 in 90 or 100, and generally greater. The institution of cottage hospitals would no doubt greatly lessen that mortality, and he hoped some day to see them established. Out-door institutions, giving home attendance, such as the Royal Maternity Charity, presented a much smaller mortality. In that Charity only 1 death in 350 deliveries occurred. In conclusion, he could add, from long experience, his testimony to the great value of disinfecting injections of a proper temperature, timely resorted to, in preventing and arresting at its outset septicæmia of uterine origin in cases of putrescent lochia. But they came too late when delirium and other formidable symptoms had supervened.

Dr. Snow Beck, in reply, observed that lying-in women were no doubt liable to the various zymotic diseases; but when they did occur they presented the same essential characters as at other times. Nothing could be more erroneous than to term every fever and every disease which occurred after parturition "puerperal fever." Typhus fever, scarlet fever, &c., had no doubt again and again been introduced into lying-in hospitals, and decimated the inmates; but the destroying agent was still typhus fever, scarlet fever, &c. Suppose the transformation theory was adopted, it would lead to this: that pregnancy had the marvellous power of transforming every disease it might be associated with, not into so many other diseases, but all into one and the same disease—puerperal fever. Such conclusions could only be admitted after the strongest proofs, and not on the mere assertion of anyone. Puerperal fever had been correctly compared to surgical fever, seeing that they both arose from the same cause—purulent infection of the general system. In primiparæ the uterus often contracted very languidly; and when it did contract well, and expel both the fœtus and the placenta, it was far from uncommon for it to become gradually

relaxed; and it was this relaxation which caused the sinuses to become so patent as to admit of purulent infection through them, and which required therefore to be so much guarded against. There could be little doubt that many diseases which had nothing to do with the pregnant state were included under the term "puerperal fever;" also that purulent infection of the general system through the pervious sinuses gave rise to one of the most dangerous affections following parturition. But whether these propositions included all the phenomena observed in puerperal fever, or not, could only be determined by the careful clinical observation of facts, and equally careful examinations after death.—*Lancet, Dec.* 28, 1867, *p.* 805.

95.—"CARBOLIZED SPONGE TENTS."—ON THE DEFECTS OF ORDINARY SPONGE TENTS; AND ON AN IMPROVED METHOD FOR THEIR MANUFACTURE.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

The use of compressed sponge for the dilatation of the cervix uteri has become so very general, that the subject of this paper may appear almost trivial in its character. But in my opinion the grave objections which exist against the employment of those in common use fully justify an earnest attempt, either to remove defects, so generally complained of, or to produce a substitute at once as cheap and effective, but also free from the bad qualities possessed by the sponge. An eloquent writer on Uterine Surgery says, "He who gives us an efficient, pleasant, and cheap substitute for sponge tents will confer a great boon on surgery. I know," he adds, "of no competent substitute, or I would be too willing to adopt it." It was expected that the sea-tangle tents would have served this purpose as efficiently as the sponge. But the experience of many obstetrists and my own, has shown that they are far less efficient as dilators, and but little less offensive in their employ than the material for which they are proposed to be substituted. In addition to which they are not easily secured in position, and are apt to be ejected from the canal of the cervix before their work is properly accomplished.

I must fully agree with the eminent authority already quoted as to the excellent qualities of sponge as a dilating agent, and I believe that no substitute for it is as yet known to us which can compare with it in its applicability to the end in view. This material is so flexible, elastic, cheap, and handy to use, adapts itself so perfectly to the varying calibre and curvatures of the cervical canal, that it is much to be regretted, that in actual use

it is attended with such disgusting and dangerous properties as to make us eager for the discovery of any efficient substitute. In addition to its other good qualities is this, that when properly made it remains *in situ*, and requires no tampon to keep it there, or at any rate none larger than a small pledget of cotton wool.

But the sponge tents sold by the instrument makers, even by such as have, as we would suppose, the largest experience in their manufacture, are so clumsy and ill-proportioned as to be really a discredit to their producers. Some of those which I have obtained from Edinburgh, and which were manufactured by the eminent chemists instructed by Sir James Simpson, are as bad as any I have procured at places of less renown in London. They are seldom sufficiently compressed to be of as much dilating power as they really might possess; and their length is so extravagantly great that it is impossible to imbed them entirely within the cervical canal without causing a considerable portion to project far into the proper uterine cavity—a most objectionable circumstance. And if they cannot be thrust up to this extent, then the lower portion projects itself into the vagina, and owing to the form of the tent, the rest generally follows and is found some hours after, lying in that canal instead of in its proper place.

The common sponge tent is not merely liable to this defect of want of proportion to the canal we propose to dilate by its means, but also by its figure it is incapable of answering that purpose as it ought to do. It is frequently made of so extremely conical a shape as to facilitate, in fact, to invite its own rejection. It is saturated with tallow and wax, and a thick layer of this compound covers the surface. Since this material does not quickly melt at the temperature of the body it follows that the sponge tent coated with it becomes really a slippery wedge, which refuses to be retained in place unless propped up by a plug. If the material were more quickly melted, then the sponge would at once adhere to the sides of the cervical canal and keep its proper position. The addition too of a filthy and greasy double fold of tape, to be allowed to hang out externally, appears as though it were designed to invite the premature removal of the tent by its impatient wearer.

With an instrument thus constructed, and so ignorantly formed, it is not surprising that the surgeon often bungles over the simple business of inserting the tent. Often, indeed, much pain is produced by the clumsiness of the thing, and especially when to keep it *in situ* we attempt to press it high up into and through the *os internum*. But defects of the kind here named are easy to be corrected by any one who will take pains to adapt his instrument to his case. And there are probably few members of this Society who will not agree with me in the

opinion that there is an almost infinite variety in the size, capacity, and conformation of that portion of the body for which this instrument is designed. A sponge tent perfectly well adapted for one variety of the cervix uteri is entirely unfit for another. In this, indeed, this material, sponge, displays its superiority over all its substitutes—that it can so readily be fashioned to meet the requirements of the individual case to be dealt with. And in sponge tents properly manufactured, of a suitable description of sponge, of an appropriate length and figure, adapted to the precise circumstances of the canal proposed to be operated upon, we possess, as it may be thought, all the qualities necessary to us.

But there is a radical defect in sponge as a dilating material, which is so serious as almost to neutralize its value. This is the horrible facility of decomposition which it seems to confer upon the discharges of these passages. No surgeon can have used sponge tents to any extent without a fear of the result to the patient in the production of blood-poisoning. The disgusting grave-like odour which is generated by them, after a few hours' residence in the cervical canal, can only be fully appreciated by those who have frequently removed them in this putrefying condition. It is horrible alike to the surgeon and to the patient, who is frequently distressingly conscious of this impurity. The cause of this decomposition seems to have escaped the notice of most observers—at least I have never seen it alluded to. There can be no doubt it is really due to the atmospheric air entangled in the interstices of the sponge, which, favoured by the high temperature, reacts upon the mucous and albuminous constituents of the discharges.

It is in consequence of the existence of this most offensive and dangerous property of sponge that surgeons have objected most strenuously to its employment. And the tents of the *Laminaria digitata* are undoubtedly less liable, on the whole, to this objection. There can, I think, be little doubt that serious if not fatal results following after dilatation with sponge tents may be attributed to this proclivity to decomposition, and though to some degree an advanced stage of putrefaction may be prevented by a more frequent change of the tents—still the retention of them only for a few hours is sufficient to commence it—and may be enough to excite mischief in a delicate and unhealthy subject.

In reply to these observations it may be said that the sponge tents of Dr. Marion Sims, prepared by soaking sponge in mucilage of gum arabic, and then drying, are properly manufactured, of convenient sizes and shapes. The objection just raised applies, however, equally to this as to the other kind of tents—and it is the most important of all. In addition, the

tents made on Dr. Sims' plan have certainly not succeeded as well in my hands as in his own. They are hard, inflexible, and almost resemble the sea-tangle tents in their density—and they possess a configuration which by no means represents that of the canal for which they are designed. The mucilage, also of which they partly consist, is extremely apt to be acid in its quality, and thus to act as an irritant of the surface to which it is applied. They have the advantage, however, over the ordinary kind, of being easily retained in place, and they have no dependent strings attached to their base.

Having in vain sought to discover a substitute for sponge as a basis of the dilating tent, I have, nevertheless, succeeded in producing by its means a dilator, to which, I think, few if any of the objections hitherto brought against sponge can be truly applied. I submit to the notice of this meeting the specimens of carbolized sponge tents on the table, and I believe they will be found to fulfil every object for which sponge is valued as a dilator, without its general disadvantages and defects.

The carbolized sponge tents possess the following properties :—

1. They are sufficiently firm to admit of easy introduction, and yet pliant so as to adapt themselves to the varying curvatures of the cervical canal.

2. They are prepared very quickly, of cheap and simple materials, and may be made by any surgeon for his own use with great facility.

3. They take firm hold of the cervical canal, and require no tampon to keep them in place : and are easily removed with a dressing forceps, the tape being dispensed with.

4. They act with great efficiency as dilators—two or three in succession being generally sufficient to open the cervix so as to admit the finger.

5. *They remain perfectly inodorous, and exhibit no sign of putrefaction during their retention* in the uterine canal.

Their properties are due—1, to the peculiar, half-ovoid shape of the tent; 2, to the investing material, the theobromic oil ; and 3, to the addition to the latter of a certain quantity of pure carbolic acid. In shape they have a rough resemblance to the cervical cavity, and are not acute pyramids or elongated cones like the ordinary tents—they are thus more readily embraced and retained. The coating employed is hard yet possesses a certain pliability, and very quickly melts at the temperature of the body. The well-known properties of carbolic acid, as a disinfectant, confer on these tents their most valuable attribute of resisting decomposition. In the quantity which I recommend to be used, this addition does not irritate the surface to which it is applied.

They are thus manufactured—I shall be happy at the close of the meeting to give any practical information on this point to the members desiring it, my object being entirely a practical one in submitting so homely a subject to this Society :—

Sponge, which must not be of too close, nor of too cellular a character, may be had in small pieces, of the chemists, at a very small cost. The larger pieces are neither so good in quality nor so adapted for our use, and they are much more expensive. It must be well washed in several waters until all dirt, sand, and fragments of stone are removed from it. It is then to be very thoroughly and carefully dried. It is to be cut into pieces of about two inches in length, and of such a shape as to produce, when rolled tightly over with tape, a shape somewhat fusiform in its character. The pieces will be cut thicker or thinner according to the degree of dilatation intended to be effected. For the largest sizes they will require to be as thick as two fingers, or even larger. For the smallest—about the thickness of the little finger.

It is important to remember that the canal we propose to dilate by this means is not generally larger than from one inch and a quarter to an inch and a half—and is often not so long. It will therefore be necessary to have some of the pieces much shorter than two inches, but very rarely, and only in abnormally elongated cases, longer than this. The sponge tents which have been supplied to me by some chemists have been in several instances nearly three inches long—and would, if I had ventured to use them, have extended from the os uteri to the fundus of that organ! I have been obliged to cut down these most clumsy contrivances to bring them to a proper size.

At any tool warehouse a slender square-sided instrument may be got, called a "broach," and this forms an excellent axis on which to roll the sponge. On a round wire or other tool it constantly slips during the making. On the table are specimens of the kind I find most useful. Inserting this pointed tool in the centre of the piece of sponge so as to form its long axis, one of the cut edges is to be forcibly squeezed down and pressed under the opposite, which may be made to roll over it and hold it down.* A piece of narrow tape is then to be rolled very tightly over the apex and brought up in regular spirals, lying close to each other, to the base, where it is fastened off. The fusiform shape is to be carefully preserved in binding the tape over the dried sponge. The tent is now ready for the first coating.

The best material for this part of the process is the cocoa-butter, or oil of theobroma. It has the advantage of being very firm, and so holding the compressed sponge well together, and

* In the centre of the sponge I always place three or four strands of common cotton wick which have been dipped previously in pure carbolic acid.

it so quickly melts at the temperature of the body that the sponge tent immediately takes to its work, and does not slip from the canal. In general it is useful to add a few grains of white wax—from three to five—for every drachm of this oil. And in hot weather this is the more necessary. But I object to any larger addition, since it elevates the melting point, and vitiates the action of the tent.

In order to confer on these tents their peculiar disinfectant character, I have with great success, added to each ounce of the theobroma oil, one drachm and a half of pure carbolic acid. The effect is most remarkable; the tents are withdrawn as devoid of offensive odour as when they were introduced—and by this means I believe the great desideratum has been accomplished. It is certainly true that no longer can the sponge tent, thus prepared, be regarded as a disgusting, or worse, as a poisonous agent. The carbolic acid, in the proportion given, has no injurious effect, in the cases I have observed, upon the surfaces with which it is brought in contact—and these proportions are sufficient. If used in larger quantity it is very probable that some irritation and soreness would arise.

The tent is dipped in the butter thus prepared, and kept in a fluid state either over a small flame or in a hot-water bath. It is simply immersed in the melted oil for a few seconds, so as to saturate only the outer layer of the sponge, and this it does, penetrating readily through the tape. The tent is then placed in a very cool place, and after it has solidified the tape is cut and unrolled. All that is now necessary to complete it is to dip it once more into the same preparation—which for this latter purpose must be much cooler than before, and of about the consistency of cream. By this means a smooth and uniform covering is given to the tent, and it is ready for use. The tents thus prepared, must be kept in a wide-mouth stoppered bottle, so as to preserve their antiseptic properties.

I may observe, in conclusion, that since the carbolic acid answers every purpose for which we require it, I have not attempted the use of other disinfectants. But it would be difficult to find a substitute for it, since many of the disinfecting agents act as astringents—whereas we need the very opposite quality. And the carbolic acid unites perfectly, and in any quantity, with the cocoa butter, and does not in any way interfere with the dilating properties of the sponge.

The carbolic acid which I use is the pure, crystalline kind. It is readily melted by gentle heat, and mingles with the oil at once on being stirred into it while fluid. It may be convenient to some of the members of the Society if I state that Messrs. Bradley and Bourdas of Pont Street, Belgrave Square, under-

take to supply medical men with these carbolized sponge tents at a very moderate price.

For placing the sponge tent *in situ*, I have constructed for myself, out of a common uterine sound, the simple and excellent instrument now shown to the Society. It is formed by merely soldering a small fillet of copper plate an inch from the sharpened end of the sound. The sponge tent is carried, its base resting on this plate, to its position, and by a touch of the forceps it is left behind, the sound coming away with great ease. The engraving represents this instrument, and shows also a slender steel wire running down its shank. This wire is for detaching the tent after it has been introduced into the cervix. By pushing it upwards it lifts the tent off the end of the sound, and the instrument is then withdrawn. This arrangement is handier in use than the forceps. Messrs. Mayer and Meltzer, of Great Portland Street, make this instrument.—*Obstetrical Transactions, Vol. IX.*, 1868, *p.* 121.

96.—ON A PLAN FOR DEODORIZING SPONGE TENTS.

By Dr. J. H. Aveling, Senior Medical Officer to the Sheffield Hospital for Women.

[Dr. Aveling has been working at the same object as Mr. Ellis, whose paper precedes this, but has accomplished it in a different way.]

I have been trying permanganate of potash. Messrs. Cubley and Preston, druggists, of Sheffield, have prepared deodorized sponge tents for me in the following way :

A conical piece of solid Turkey sponge is transfixed in its long axis by a silvered knitting needle, and tightly bound round with twine. When dry, the needle is withdrawn, leaving a tubular opening throughout the whole length of the tent. After the tent has been filed into shape, and rubbed smooth with sand-paper, its smaller end is sealed with a drop of melted white wax, and it is then ready to be filled. This is a delicate operation. The permanganate of potash must be very finely powdered, and dropped into the tent in extremely small quantities, and shaken down carefully, or the tube becomes clogged. When full, it is closed with another drop of wax, and having passed a small thread of silk through the end of it, the tent is ready for use. I have had them made of various sizes, and numbered according to their lengths as follows :

Number .	1.	2.	3.	4.	5.	6.	7.
Length .	1 in.	$1\frac{1}{4}$ in.	$1\frac{1}{2}$ in.	$1\frac{3}{4}$ in.	2 in.	$2\frac{1}{4}$ in.	$2\frac{1}{2}$ in.

This plan of numbering sponge tents according to their

dimensions may be useful both in ordering the size of tent you require, and in describing how large a tent has been used in the treatment of a case.

I will merely add that the deodorization takes place in the following manner:

Until the tent has dilated to its very centre, the charge of permanganate (about two grains) remains undisturbed. It then gradually dissolves, and in solution renders the tent so perfectly sweet that upon its removal no unpleasant odour can be detected.—*Obstetrical Transactions, Vol. IX.*, 1868, *p.* 267.

97.—NOTES OF AN INTERESTING CASE OF MIDWIFERY.

By Dr. W. H. Taylor, Anerley.

On the 22nd of April, 1865, I was called to attend a woman, aged 29, in labour with her first child. Finding the pains were very slight I left. I called again in the afternoon, and made an examination, but could not feel the os. I began to suspect there would be some difficulty in the case, but, as the pains were about the same, I went away, telling the nurse to send for me as soon as they became stronger. About 8 p.m. I was summoned, and now found the pains were of the right sort. I made an examination, but could neither detect any presentation, nor find the os. I felt about for some time, and at last detected a very small indentation (not an opening) about the size of a pea. This I considered must have formerly been the os, and that I had a case of occlusion of it. I also found the pubes projecting very much inwards, making the antero-post diameter very narrow, and altogether rendering it a case in which I did not feel justified in acting without a second opinion. I therefore sent for my friend, Dr. Stilwell, of Beckenham. He came, and agreed with me as to the nature of it. We also came to the conclusion that nothing remained but to make an artificial os, and overcome the pubic projection by forceps or craniotomy. I informed the husband of the difficulties of the case; he suggested further advice from town, but I told him the confidence we had in each other by reason of our having acted together in several most difficult cases which had occurred in our respective practices, made us feel quite equal to any emergency, and that we declined the assistance of a physician-accoucheur from London.

First, to make an os, I passed one of the blades of a pair of long scissors through the indentation in the uterus, and made several notches round it, which enabled me to introduce the point of the forefinger into the uterus. We then waited to see the effect of the pains. It did not dilate much. Then, by degrees, I introduced the middle finger, and by stretching the

edges, somewhat increased the opening. After three hours' work I managed to get two fingers in, and could feel the head presenting. After manipulating for another two hours the opening was sufficiently dilated to render it advisable to rupture the membranes. This being done, the head, after a time, came down as low as the pubic projection would admit; but here all further progress was stopped. We then applied the forceps, but there was not room enough, and we had to give it up. There was now no help for it but to perforate the cranium. This we did, and after great difficulty succeeded in delivering the woman. There being some placental adhesions, we broke them down, and removed the placenta; and heartily glad we were to complete the case. It was now 8 a.m., and we were somewhat fatigued with our anxious night's work.

The most gratifying part of the case was that the patient recovered without a single bad symptom, would insist upon getting up on the seventh day, and was about as usual in a fortnight. Since then her health has been good, the catamenia regular and without pain, and she is now in the third month of pregnancy. She has left the neighbourhood, but will return to it to be under my care for her confinement, and I hope, by inducing labour before the full period, to deliver her of a living child.—*Lancet, Jan.* 11, 1868, *p.* 41.

98.—ON GRANULAR INFLAMMATION OF THE CERVICAL CANAL OF THE UTERUS.

By Dr. Fleetwood Churchill, President of the King and Queen's College of Physicians in Ireland.

[Granular ophthalmia is the best illustration of granular inflammation of the cervix which can be given. When examined carefully by sunlight, the lining membrane is generally found bright red, or, if much congested, of a purplish red, colour; and we can distinguish small granulations scattered over or covering the surface.]

Sometimes we find a granular condition of the cervix, with or without erosion. I have generally observed more or less vaginitis; it may be general or in patches; but I have rarely seen the vagina perfectly free from inflammation. I am not prepared to say whether this inflamed condition of the cervical canal is always a consequence or continuation of inflammation of the mucous membrane of the vagina, or a portion of it; but I think it very probable. I have, at any rate, seen granular vaginitis along with the same affection in the cervical canal.

Another symptom is, I think, quite characteristic of this disease—viz., a wide open os uteri. Sometimes the finger may

be passed as far as it can reach; in other cases only the point can be introduced; but, with very few exceptions indeed, it is much more patulous than natural. Now, in all these cases of patulous os uteri, if you carefully examine the mucous membrane, you will find intense redness and granulations, except at one or two places, it may be, from which the mucous membrane has been stripped.

But you will naturally ask me, how we are to examine the inner surface of the canal. There are two means of doing so.

1. By the speculum with bright sunlight. The cervix uteri is always more voluminous than usual, and can hardly be embraced by the end of the speculum; but, when brought as centrally as possible, the pressure closes the wide os uteri, and it appears simply a prolonged slit. If one lip be separated from the other, by a slight hook—I use the one with which Mr. Spencer Wells raises the peritoneum in the operation of ovariotomy—we can quite easily see into the canal a quarter or half an inch; and, if so much appear inflamed and granular, we may fairly infer that the remaining portion is so likewise.

2. By the endoscope. I am indebted to my friend, Dr. Cruise, for my first view of the cavity of the uterus and for instruction how to use the instrument. He has added to my obligation to him by promising to attend here and give information as to its use to any one who may wish to understand it. I have since repeatedly used the instrument, and by it confirmed the diagnosis I had made from a sunlight examination of the lower portion of the canal.

In most cases the canal is open enough to admit of the tube being passed with great ease through it into the cavity of the uterus; in some cases, it would admit a much larger tube. If, however, it should be too narrow, nothing is easier than to dilate it with a tangle tent or prepared sponge to any extent we please. Having passed the tube to the fundus uteri, sponged it out, and made our observations, we may turn its orifice to different portions of the cavity, and then, slowly withdrawing it, we can examine each successive portion of the canal, and there will be no difficulty in distinguishing the inflamed from the healthy portions. The amount of information we obtain, or could obtain, through so small a tube (and I have no experience yet with the larger) is not great. I think the plate given by Dr. Cruise of the inflamed mucous membrane of the bladder gives a sufficiently correct idea of this granular inflammation of which I am speaking. Portions are bright or deep red, and rough; other parts are paler and smooth; except in the severe cases, where we find no healthy portions of mucous membrane.

I have mentioned that I have seen a few cases in which the os uteri was not more open than usual, and in which I ascertained that granular inflammation existed. In such cases, I have observed that the introduction of the sound in the gentlest manner caused some bleeding, or, in other cases, gave actual pain. If, in such cases, the canal be dilated with a tangle tent, its granular condition may at once be ascertained; so that I regard these two symptoms—slight bleeding or pain when the sound is passed very gently—as conclusive. Reasoning *à priori* I think we should expect to find the canal narrowed or closed, as the ordinary result of granular inflammation; but the opposite is the case in the vast majority of instances. Nor am I able to offer any scientific explanation; all I know is the fact, that of this disease the most unvarying accompaniment is an os uteri unusually patulous.

I have been thus particular in describing the anatomico-pathological peculiarities of the disease, because, as one might expect, the symptoms (properly so-called) are less distinctive, but have much in common with other affections of the uterus. There may be, and generally is, the usual aching pain in the back, with, possibly, some feeling of weight or bearing down. The menstrual flow is almost always increased, sometimes excessive, or recurring at short intervals and from slight causes. I recollect only one case where the opposite extreme was observed. There is always more or less leucorrhœal discharge, thinner, and white or yellowish, if there be vaginitis; thick and tenacious when the disease is limited to the canal of the cervix. When the speculum is used, we may see this stringy discharge hanging out of the os uteri, and it is sometimes difficult to remove it. When the disease is of long standing, it interferes, more or less, with the general health, giving rise to languor, listlessness, loss of appetite, and imperfect digestion. In addition, it is not less potent than other disorders of the uterus in exciting reflex irritation, of which, perhaps, the most common is ovarian pain and irritability of the bladder; very distressing while they last, but not absolutely requiring any special treatment beyond a vaginal suppository of belladonna or morphia, and which disappear as the cure of the disease progresses.

Upon one uterine function, gestation, the influence of a severe attack of granular inflammation is apt to be disastrous. It does not always prevent conception; but, in a great number of cases, it causes abortion. I have known a succession of miscarriages apparently result from this cause alone; and I think that, in all cases of the disease, no risk of impregnation should be incurred. It will be wise also, in any case of abortion, to examine into the condition of the canal after the lochial discharge has ceased.

Diagnosis.—After what I have already said, I need occupy very little time with the diagnosis. Our suspicions will probably be first excited, when making a digital examination, by finding the point of the finger pass more or less freely into the os uteri, and the inference from this will be turned into certainty by a sunlight or endoscopic investigation. A little longer process will be necessary when the os is not patulous; the occurrence of pain when the wound is passed, or a drop of blood escaping afterwards, leave but little doubt, and that may be removed by dilating the os with tangle, and a subsequent careful examination.

Treatment.—The disease is curable enough, and the course of cure steady but slow. It must be for such cases that the different kinds of curette have been invented, to scrape off the granulations. This does not appear a very scientific procedure, and I am sure you can cure this diseases as effectually without it. The endoscope affords us a very nice and precise mode of applying remedies to any part, either of the cavity of the uterus or the canal of the cervix, without the risk of doing more than we intend, so that we may safely use stronger remedies than we dare in any other way. By this means, strong nitric acid, tincture of iodine, or lunar caustic, may be applied when we please, and to a greater or less surface as may be necessary, or if we wish to limit our treatment to the cervical canal, a smooth piece of deal may be dipped in the acid or the tincture, and freely applied. I have found it better to begin with a free application of strong nitric acid, so as to destroy the granular surface, and this may be repeated at intervals of three or four days, until we obtain a smooth surface. The vagina should be syringed with cold water, or camomile tea, one or twice a day. After this, I dip a small twist of cotton-wool in tincture of iodine, and apply it to the entire diseased surface, twice a week, continuing the syringing as before. As the cure progresses, the lining membrane becomes paler and smooth, and the os uteri closes somewhat, but it is not restored to its natural size for some time. I have found a strong astringent applied to the inner surface, in the way I have described, the best aid, and with this view I have used with benefit both the pernitrate and perchloride of iron, at the same time syringing the vagina with a strong astringent.

I should strongly advise "separation" until the treatment is finished, as conception might seriously interfere with our procedure.

There is a disease which I meet with occasionally, which may possibly have some relation to the foregoing, but of this I am not sure. I mean a vascular tumour at the os uteri, exactly like that we find at the meatus urinarius. It varies in size from

a pin's head to a raspberry, and, unlike the small polypi found in the same situation, bleeds on the slightest touch So far as I know, it gives rise to no symptoms but menorrhagia. It can seldom be recognised by the touch, because of its small size and softness, but it is easily detected by the speculum. The cure is simple—excision by scissors, or Dr. Braxton Hicks's small wire *écraseur*, or torsion by Wilde's snare, and, subsequently, repeated cauterisation.—*British Medical Journal, Jan.* 4, 1868, *p.* 2.

99.—REPORT OF FORTY-ONE CASES OF UTERINE POLYPI.

By Dr. J. Braxton Hicks, F.R.S., Physician Accoucheur to Guy's Hospital.

[In all the forty-one cases treated of in the following article, the polypi, which were of various forms and very different sizes, were all removed by the annealed wire-rope écraseur. No death occurred in the whole series, and the removal was not followed by hemorrhage, nor by symptoms which gave any anxiety except in two instances. One great point insisted on in all these cases, was the keeping of the patient in a state of quiescence for some time after the operation—generally at least a fortnight.]

Another point in the treatment of these and similar cases is the removal of secretions by warm vaginal douche twice at least a day. After these operations small clots of blood, or retained secretions, with perhaps, a fragment of dead tissue, are liable to set up decomposition rapidly, and the natural evacuation of them is hindered by the dorsal position of the patient. Hence a pint of warm water thrown up the vagina twice a day, with or without a little disinfectant, is very useful in preventing, to say the least, minor symptoms of irritation.

In the whole of these cases the polypus was removed by one method, namely, by "écrasement." And this mode of separation has an advantage over section by knife or scissors, independently of the less risk of bleeding—I mean that by the crushing process we have a line of condensed tissue, which doubtless forms a barrier to the ready absorption of unhealthy matter. This is the same, only mechanically induced, as is produced by the actual cautery (as by the galvano-cautery); but as there is in these cases but little advantage in the latter over the former, and as the application of the wire-rope écraseur is much easier, it seems to me that it is generally preferable.

So far as my own experience goes, I believe that the instrument which I described in a former volume of these Reports will be found the most universally applicable of any kind. Although I do not wish to insist upon this point with undue

strength, because an instrument to which one may have become used is handled with more facility and adaptability than any other, yet inasmuch as it has been tested, not only in these forty-one cases, but also in some twenty more, under my colleague, Dr. Oldham, in the hospital, and has always, with one exception of enormous fibroid, where the chain also would not do, been equal to the task of removal with ease and rapidity, I think I may, without undue confidence, make the above statement.

Under these circumstances it may be well to give a few practical remarks on polypi, and on the way I have adapted the instrument for their removal.

Polypi come before us practically under four conditions—

A. *External to the os uteri.*

1. When sufficiently small to be included in the speculum.

2. When a loop of the écraseur can, without much difficulty be passed round the polypus by the fingers or hand only.

3. When their size is so great that they occupy the vagina so completely that the hand cannot reach the pedicle.

B. *Within the os.*

4. When they are within the canal of the cervix, or within the body of the uterus.

The *first* class are easily manageable; They are readily seen, readily caught and snared, and easily severed. A medium-sized instrument, one with a rope of ten to fifteen strands, is sufficient; and the smallest kinds of all may be removed by the smallest instrument, with three strands to a rope. It is best not to underrate the tenacity of the compressed tissue, and it will always be satisfactory to have more power than is really wanted.

As a rule, these smaller and generally more vascular polypi bleed most after division, partly because of the relatively greater amount of blood-vessels, partly because the line of écrasement is narrower, and because the force required to crush is less; therefore it is well to have a styptic at hand to stop any bleeding, not that it is ever really much, but that the operation will be more complete to the appearance of bystanders; and the employment of a styptic to the base and surrounding parts is beneficial to get rid of abrasions, and the irritation caused by the presence of these growths.

Now, in the *second* class, we are obliged to dispense with the speculum, and trust to our hands only. We thus lose two advantages; the first, the assistance of our sight to quickly seize the growth; the second, the protection which the speculum gives to the vagina from the contact of the fingers and the instrument for removal.

But there are some cases in which, although from its small size the polypus would readily fall into the speculum, still the external parts may be so rigid and undilatable as to cause so much resistance to the speculum, that the instrument, and finger to guide, can be passed less painfully than the speculum. A few of the cases recited were treated in this way; they occurred, as may be surmised, in single women, somewhat advanced in years, where much hemorrhage had not occurred to lessen the tone of the system.

In the second class of cases, then, we may pass a noose of the écraseur slowly through the vulva, in such a manner as to give the least pain and compression; the tip of the fore, and, if possible, that of the middle finger also being placed just within the loop at the part opposite the eye of the instrument. These fingers should carry the loop to the back of the vagina, while the instrument should be carried towards the front. The loop can then readily be placed round the polypus by very simple adjustment. As soon as the loop has passed the equator, or greatest diameter, it is to be made smaller by pulling down the end, if still unfastened, or by screwing it down, if it is. By this process, the loop glides very easily to the base, the instrument, of course, being simultaneously pushed up, as far as possible (and generally this can be done more quickly than the description can be read); and then the ends having been properly secured, the screw can be worked, and the pedicle crushed through. The rope requisite for this class of polypi should be formed of from twenty to thirty or forty strands. Better too strong than too weak. It will also be more readily introduced if it has a certain degree of stiffness.

The force used to crush the pedicle is sufficient to prevent hemorrhage; at least in all the cases I have had no such contingency has troubled me. It would be well, however, to have the speculum and a styptic at hand in case of such an event occurring.

After the severance of the growth we have to remove it from the vagina. This is, generally speaking, easily done in this class of cases, by passing two fingers above it, and hooking it down; still, sometimes, in the larger sort, or where there is a contracted outlet, it is requisite to have a vulsellum forceps, or the whalebone fillet at hand.

The *third* class requires the largest sized instrument for their removal, not so much for its strength, perhaps, as for its length, because it is requisite the base should be reached, for we generally find that in this class the pedicle is by no means so very large. The growth has generally been for some time outside the uterus, which is pushed upwards, sometimes as far as the umbilicus, sitting as it were on the top of the much larger polypus, or

pushed aside. To reach to the neck of these long growths requires a considerable length of shaft in the instrument. If we do not reach the base, the point at which the section is effected is sometimes so large as to make it impossible to divide it. This is especially the case with fibroid polypi; even the chain écraseur is unable to pass through them, the chain breaking, or remaining imbedded in the growth; but no such strain need in this class of cases be put on the instrument. We can get over the difficulty by passing the loop of rope past the equator of the polypus; then draw it smaller, pushing at the same time the instrument higher up the vagina; by this simultaneous movement the rope slips up to the origin of the growth.

The best mode to effect this movement is as follows: Take the écraseur, with the rope attached to the traversing hook, the other end free, and make a loop of a size about sufficient to pass round the polypus. Then take two slightly curved canulæ, and through each of them pass a small wire; when this appears at the top, pass the end round the rope and back again through the canula, till it appears again at the lower end; the two ends then to be fastened off. Each canula is to be placed on the loop in such a position that it is about the same distance from the other as from the shaft of the écraseur instrument. The loop is then passed into the vagina posteriorly, assisted by the canulæ as handles. When the loop is wholly within, the écraseur is also passed in, but upwards in front, the finger assisting to adjust it over the polypus. The canulæ are then pushed carefully upwards, till the root is reached. Then the écraseur is also pushed up in front, the free end of the rope at the same time being drawn down gently, so as to lessen the size of the loop. When the instrument is thus adjusted, fasten the free end of the rope off securely. Then remove the canulæ by unfastening the wires and pulling down one end of each. This pulls the wires off the rope; the canulæ are then pulled down. Upon turning the screw the pedicle will be cut through.

Some instruments have been made to work with an unlimited quantity of rope, but this is really unnecessary. It is very rare to find a polypus whose base is more than three inches across. This will give a circumference of nine inches. If we draw the rope tight before it is fastened off, we might deduct from this an inch, so that any instrument which can bring the hook down over eight inches is sufficiently long.

Formerly I advised the canulæ to be passed up together behind the growth, and then brought forwards, running their little noose along the rope which they thus carried round the neck, but I find the plan just recommended more simple and manageable. The curving of the canula is necessary in these large polypi, because it adapts itself to the ovoid of the polypus,

without injuring the vagina (a male catheter cut off at the end, and stiffened by a stylet a slight degree shorter, would answer instead of a metal canula).

When we have separated these polypi, they require some degree of management to remove them from the vagina. A pair of midwifery forceps, or the whalebone fillet, will generally succeed. The vulsellum forceps also will assist, according to circumstances, but they require much care in their use. On the removal of polypi of this class I have not seen any bleeding to follow from écrasement.

The *fourth* class of polypi are those which give most trouble, and require most care, both during operation and after— I mean those which have not escaped from the uterus. When they are of large size, they are generally attached rather high up towards the fundus uteri. It has been considered a rule by some that as soon as a polypus appears through the os, or as soon as it is possible anyhow to diagnose it, it should be removed. Doubtless this is an excellent rule so soon as the growth has become really a polypus ; but supposing the base is still nearly as large as the equator when we proceed to remove it, we find either that we have to cut through a great width of tissue, rendering absorption of deleterious matter more probable, and leaving a portion of the growth behind to form a source of future trouble ;—or that during écrasement we draw together, or pucker up the wall of the uterus, leaving a chance open for the supervention of immediate and future mischief.

The difficulty of removing these forms very early in their imperfect state is, of course, greater than in the true polypus form, because of the trouble one has to keep the ligature on ; but the danger is still greater than the difficulty of removing. Those that are really polypi are very easily removed (although the difficulty has been dwelt upon by some more than it need be), so soon as the os is large enough to permit their exit when severed, if the same plan of introducing the loop be adopted as above described, substituting for the vulva the os uteri. Sometimes the growth is attached to the side of the uterus ; this will be found out by the sound, and would require that the canulæ should be carried up together to the fundus, separated and brought down across the neck of the growth. But the smaller sort, springing from the upper cervix, do not distend the cervix enough to permit the loop of rope to pass up between them and the walls of the cervix, and therefore give some trouble. This, however, is not very difficult to manage. Use a rather stiff rope ; make a loop as nearly the size as possible ; introduce the loop into the cervix in a slanting manner, so that the part of the loop most distant from the écraseur shall pass up between the os and the polypus behind. When about half has

entered, pass the head of the écraseur within, telling the patient to bear down ; this generally drives the growth into the loop, which then can be tightened. If the rope is very limp, it gives some trouble ; in fact, this kind of case generally is the most trying for flexible wire.

Should this plan not answer, the loop might be carried up with one canula as above directed, or a single stout wire might be substituted for the flexible rope. However, I have always managed with the latter, although with some little trouble. Further enlargement of the cervix by the tent would give much greater facility for action.

For the severance of all tissues by écrasement it is very important to remember the excessive resistance which is given by the condensed tissue, particularly the less friable kinds, as areolar tissue. The chain écraseur having angles succeeds in fraying out some fibres, by which gradually the tissue yields ; with the single wire there is no such assistance, it acts simply by crushing, and this more particularly if both the sides come down together ; when one side only comes down, there is a certain amount of torsion which lacerates and assists ; this is increased by the alternate bringing down of each side. In the rope of many secondary strands we have an intermediate condition, the fraying power of which is much increased if the rope be made rather irregularly. I have found a further assistance by winding round it two small flattened wires of the same material (annealed steel), crossing their spiral. The edges of this wire, at so many points, produce sufficient fraying, and allow us to use the flexible rope in any position required.

These practical points having been attended to, I believe the kind of instrument with which these polypi have been removed will be found sufficient in almost every case, and remarkably easy to apply. Some have complained that the rope breaks. Upon inquiry I have found that the rope has been put to work for which it was not intended, or that it was of so small a size that it was no wonder it gave way. Occasionally the annealing has been carried too far, so that the resistance of the wire to tension was reduced. A very common error in makers has been to make the eye of the instrument with sharp edges, as if on purpose to cut the rope ; or they have left the hook with so shrrp a bearing that it would certainly cut any rope. The eye, again, has been made looking upwards, whereas it ought to be set somewhat obliquely, so as to make the bending of the chain less acute, when the line of section is at right angles to the shaft.

In the cases here brought forward, as also in those of my colleagues, I have not seen the rope break more than once. The way to avoid it is to use a rope of as large a size as the case

can be managed with ; for I have found that the pedicles of the smaller polypi are as resisting as, and sometimes even more so than, those of the larger kinds.

There is one practical point which it is well to remember after the removal of these bleeding growths, particularly if the patient be at or approaching the climacteric ; and this is, that on the bleeding ceasing there is not infrequently a condition of hyperæmia, which leads to headaches, flushed face, with general discomfort, and perhaps might cause more serious troubles. It would be best to place the patient on a light unstimulating dietary, with gentle laxatives ; and, if necessary, now and then a sharper purgative.

[Dr. Hicks relates 42 cases of various kinds of polypi, which we have not room for.]—*Guy's Hospital Reports*, 1868, *p.* 129.

100.—ON POLYPUS UTERI.

By Dr. Robert Dyce, F.R.S.E., Professor of Midwifery, University of Aberdeen.

[The forty cases of polypus uteri which form the subject of the following article have all been successfully treated within the last few years. All the tumours were of either the purely fibrous or of the fibro-cellular variety, but by far the greater number were of the first class. The most ordinary size of them was that of an orange, but some were much larger, and others considerably smaller.]

The following table exhibits the various methods employed, and the number removed by each plan :—

Excision, ...	18
Excision with enucleation (double origin), .	1
Ligature alone, ...	9
Ligature with excision, ...	3
Torsion, ...	4
Enucleation, ...	3
Ergot alone, ...	1
Ergot with sponge tents, ...	1
	40

The instruments used for excision were a strong vulsellum, and equally strong curved scissors, which cut principally from near the point, which latter is rounded The scissors are ten inches long, and slightly bent ; the vulsellum of the same size, but nearly straight. The distance from the handles to the joint, in both, is 7 inches.

For ligature, the double straight canula of Neisson, improved

by Gooch and Levret, was used, and latterly an improvement was added, by which the ligature was passed around a winch, and thus more readily tightened; the material used was strong whip-cord. I once used wire, intending to cut off the tumour at once, but it did not succeed, although the process was shortened. The cord was tightened morning and evening, and the vagina syringed at the same time with tepid water. Usually in from four to seven days the neck was cut through. I never but once saw any constitutional symptoms follow, but the length of time required, and especially the care requisite to prevent injury by the patient impaling herself, and the consequent necessity of keeping her on the side during the period of its application, induced me to substitute the scissors in all my future cases.

For torsion, the vulsellum was alone sufficient; and it is far preferable to two fingers, which some recommend, because once a sufficient hold is obtained, a few turns will readily separate the tumour.

For enucleation, a scratch with the finger-nail has generally answered, and once begun, it is easily completed, provided the tumour is within reach. The rent made in the investing membrane while fixing the vulsellum, is another and ready spot for this purpose, but sometimes a slight snip with the scissors may be required. The vulsellum is here indispensible, both for dragging down the tumour as well as fixing it. This mode of operating is so speedy and so bloodless, that I always now first attempt enucleation; but in my experience, there are very few tumours so loosely invested as to admit of this mode of operating; still I think it should always be tried.

My first case was accidental. The tumour was very large, and partly intra-uterine. I excised what I believed to be the neck, as it allowed of its descent to some extent, but it was found to have another and broader attachment higher up within the uterus, within a bridge of strong fibrous tissue between the two attachments. With the first incision of this second attachment, my finger entered a vacuity formed by the tumour and its investing membrane; from this point the separation was cautiously made, and the mass was easily and quickly removed. This part of the tumour was fully larger than a turkey's egg.

The mode of using ergot will be best understood by a detail of the cases in which it was employed. The principle which first led me to employ it was, that, believing, as I do, that during every severe uterine pain in labour the circulation through the vessels of the uterus by which the placenta is supplied is entirely arrested, it occurred to me that if I could first create uterine contraction and then continue this contraction for a length of time *continuously*, I would arrest the circulation for such a

length of time as to prevent the tumour receiving sufficient or any nourishment; that the consequence would be the death of some part of the tumour; and that, once it had begun, it would go on dying and decomposing, and thus eventually the whole mass be brought away. It is on this principle of continuous action that ergot destroys the life of the child in labour, for so long as there are intermissions of ease, I never saw danger from it, but so sure as the pains are constant and without any intermission the child will die, unless immediately born. It is from the same cause that, in impetuous labour, called "tetanic," where *no* ergot is given, children are lost, the too long continued action preventing circulation between fœtus and mother, and hence due arëation of the blood for its life and nourishment. This theory I acted on, and succeeded in removing from two patients a tumour of enormous size, in a very few days, which filled the whole uterus, and distended the abdomen to a size far larger than that of a woman at the full time of pregnancy.

I conclude with detailing a few cases illustrative of the various methods adopted.

Polypus Uteri removed by ligature, showing the danger of passing the ligature too high, thus including a portion of the uterus.—Elizabeth Ad--s, aged 42, unmarried, came under my care Nov. 10, 1853. Is exsanguined, weak, and unable to make the least exertion without fainting. Only a few months before her admission into the infirmary she was examined per vaginum.

On examination, the finger comes immediately in contact with a large round, smooth, and insensible tumour, protruding through the os uteri, which is very much dilated and thin. From the small vagina its attachment can with great difficulty be reached; but by the help of some relaxation, produced by chloroform, it was ascertained to be adherent below, about two inches within the cervix at its anterior aspect, but the extent of adhesion could not be reached above—the tumour seeming to have no neck, and to spring from a broad base. On the 14th, a vulsellum was firmly fixed high up on the vaginal portion, and a strain made upon it, but without producing any sensible change. A ligature with Gooch's double canula was, with great difficulty, passed over the tumour. Immediately on its being tightened, she complained of more than usual pain, with a feeling of sinking and faintness; but as there had been so much difficulty in encircling the tumour, it was allowed to remain. She passed a restless night, with frequent attacks of faintness and sickness, with continued pain. The ligature, however, was tightened, the vagina syringed with tepid water, and an anodyne given. The tightening of the ligature daily, with the frequent

washing the vagina to remove the putrid discharge, was continued for five days. The constitutional irritation, however, had now become so alarming, attended with exhaustion, loathing of food, great thirst, and rapid pulse, that, fearing a fatal result, the ligature was removed, and she was ordered strong beef tea, with brandy and wine. The following day, though still faint and low, she had not been sick, had less uneasiness, and had taken a good deal of the stimulants. The tumour was much diminished, and the discharge continued profuse. On the second day after, or seventh from the application of the ligature, while at stool, the remains of the tumour, as large as the closed fist, came away in a putrid state. From this time she gradually recovered, though her convalescence was much retarded by an attack of diarrhœa.

This case affords an instructive lesson. In my anxiety to encircle the whole mass, which was sessile, I had included a portion of the uterine wall in the ligature, no doubt by the weight of the tumour inverting a portion of the uterus. This seemed evident from the severity of the pain, and feeling of faintness immediately induced when the ligature was tightened.

I should have removed it at once and fixed it lower down, as the success of the operation would still have been secured, without endangering the life of my patient. The late Dr. W. Hunter lost more than one patient by including a portion of the uterus in the ligature, which he ascertained by examination after death; and Gooch is very strong on this point, and expressly says that we are not to be anxious to include all the stalk, as he found it sufficient to get over the body of the tumour to insure the success of the operation; that the stalk will drop off in a few days, like the umbilical cord in a child, or be decomposed and pass away in the discharges. And I have found, in more than one instance, that even though there is no neck or stalk to the tumour, but growing from a broad base, that if the ligature can be made to include but the greater part of the tumour, the remaining portion will, in a few days, slough away, and a cure be effected. The next case illustrates this.

Large Polypus removed by Ligature, a portion of which had only been included in the Ligature.—Isabella W., an unmarried farm servant, aged 45, was admitted into the Infirmary, 24th March, 1856, with an enormous tumour distending the vagina, and pressing out the perineum, as in the last stage of labour. From its size, it was impossible to pass the finger over the tumour, or to reach the os uteri. Above the pubes a well-defined tumour was felt, and pressure on the vaginal tumour was communicated to that felt in the abdomen. She has been losing blood in more or less quantity for upwards of two years,

and though for some time under treatment, had never been examined until a few days ago. The symptoms, both constitutional and mechanical, were very urgent and distressing. Seldom could any urine be passed without the use of a long flexible catheter, and the pushing up the tumour, nor could the rectum be emptied without injections. Two days after her admission, a ligature was passed around what was supposed to be the bulk of the tumour, with more ease than was anticipated, by Gooch's double canula; but on tightening it again the same evening, the ligature broke. There had been no pain from the operation; no hemorrhage, but complete retention of urine, and some little manipulation was required to reach the bladder by the catheter. On renewing the strangulation two days after, considerably more difficulty was experienced, from the tumour having swollen, consequent upon the first tying; it was not hence possible to include so much of it in the ligature. The ligature was tightened morning and evening, and tepid water thrown up the vagina each time. On the third day from this second attempt, the discharge became copious, brown, and fœtid —proofs of its decomposition. From this time all uneasiness passed away, she no longer required the catheter, and the bowels acted of themselves. On the ninth day, finding by examination that the os could be distinctly traced, with the adherent tumour passing within the cervix, it was determined to excise what remained, but by slight traction at the canula, and some pinching by the finger, the whole came away.

The tumour was now greatly reduced in size, and much resembled an hour-glass in shape; the central narrow part, which was about equidistant from both ends, was tightly encircled by the ligature. The fœtid discharge continued for a few days; and she was discharged cured in the third week from her admission.

Polypus Uteri removed by Ligature and Excision.—Elizabeth T., unmarried, aged 47, housemaid, full habit of body, but extremely sallow in complexion from loss of blood, was admitted 7th March, 1854, with the ordinary history and symptoms of polypus uteri of two and a half years' duration. The tumour is of considerable size, round, smooth, and insensible, filling up the vagina, and descending to within an inch of the external orifice. The vagina is relaxed, hence the finger can be easily passed around the tumour, and its neck felt passing within the os and cervix, to the latter of which it is attached at its back part fully two inches within. On the 12th, chloroform was administered, which produced great relaxation of the vagina. The vulsellum, when fixed, brought the tumour still nearer the perineum, so that the ligature was easily passed over it and

tightened; when, finding that the peduncle was small (not thicker than two fingers), it was at once excised above the ligature, and the mass brought away; not a table-spoonful of blood was lost. The tampon was used for twelve hours. The tumour was as large as an ordinary orange. No bad symptom followed, and she was discharged on the 20th, cured.

With the experience I now have, this tumour might have been at once excised. By the vulsellum it was brought quite to the perineum, thus enabling the neck to be entirely embraced by the finger.

Polypus Uteri removed by Torsion.—Two cases were in connexion with pregnancy, and no doubt both of them had been the cause of abortion.

Mrs. S., mother of four children, miscarried at the fourth month, with not more than the ordinary loss of blood. The discharge had ceased, and the patient was on the sofa on the fourth day. A few days after, while dressing, she was alarmed by rather a copious discharge of blood from the vagina. She got sick and faint, but without the slightest pain. By the time I saw her she had recovered, having taken some brandy, and applied cold cloths to the abdomen. On a careful examination, nothing could be detected. The os was open, but barely admitted the point of the finger; nor did the uterus appear larger than it ought at this period of its involution. She was therefore kept in bed, and directed to continue the wet applications, and to avoid stimulants and animal food. These means were adopted lest the cause of the hemorrhage should be the presence of another ovum in utero. It could be no part of the aborted ovum, because the whole mass, fœtus, placenta, and decidua came off "entire." For the next ten days the bloody discharge, though very moderate, was nearly continuous. Tinct. cannabis gtt. xv. every four hours was first tried, then ergot in 5 gr. doses every three hours. By these means occasional uneasiness, then regular pains in her back, were produced. On examining now, the os was found more open, so as easily to admit the finger, and a smooth something was felt pressing through it. A vulsellum was cautiously carried along the finger through the os, and the tumour grasped. Torsion was made, and a firm polypus, the size of a walnut, was removed in a few seconds: from that moment the hemorrhage ceased.

A second case occurred shortly after, in August 1858, under similar circumstances. The patient, Mrs. W., had borne three children, and now had miscarried between the fifth and sixth month. The child cried, but soon died. The loss of blood was not more than usual, and had nearly ceased, but in a few days it

returned with some violence, and, notwithstanding the ordinary means, it continued to recur at intervals for several weeks. For some reason she would not permit an examination until the exhaustion and prostration made it imperative. At this time I was asked to see her along with Dr. Ogston, whose patient she was. The os was found open, so as freely to admit the point of the finger, but nothing could at first be detected; at length, by firmly pushing down the uterus from the abdomen, something was felt to touch the tip of the finger. A vulsellum was cautiously guided within the uterus on to the tumour, and with difficulty grasped. Finding no pain produced, the instrument was more firmly pressed and twisted. In a few seconds the tumour was brought away; it was firm and cartilaginous, and fully equalled a walnut. The hemorrhage ceased from that moment, and in due time she menstruated.

In this case it required very strong pressure above the pubes to bring the tumour within reach of the finger, so as to be felt, and no less care in carrying the vulsellum so far within the uterus, over so small a tumour, as to avoid injuring the uterus itself. The safe method in all cases, whatever be size of the tumour, is to press the closed vulsellum or forceps (but I prefer the former) gently and steadily up on to the tumour, and allow the instrument to expand of itself as it embraces the tumour, and not to attempt to grasp or try its hold until a sufficiency is within the blades, which can be judged of by the expansion of the handle; because once fixed, there it should remain. If this is not attended to, from the limited space and the almost impossibility of liberating both blades to get another hold, it becomes very embarrassing.—*Edinburgh Medical Journal, Dec.* 1867, *p.* 503.

101.—THE SPONTANEOUS ELIMINATION OF UTERINE TUMOURS.

By Dr. Alfred H. M'Clintock, late Master of the Lying-in Hospital, Dublin.

[To study any disease aright we should commence by studying the course which it takes when left to itself, and uninfluenced by the active interposition of art.]

The study of a large number of recorded histories, together with the results of my own clinical observation, lead me to believe that there are *five* different modes by which nature may effect the cure of a tumour of the womb. To reduce the number cannot be done, I think, without confounding cases essentially different, whilst to increase it beyond five would be devoid of

any real utility. These five modes of elimination are as follows, viz.:—

1. Interstitial absorption.
2. Simple detachment, or separation.
3. Calcareous transformation, or petrifaction.
4. Sloughing, or disintegration.
5. Expulsion by uterine contractions.

I have arranged them in this order, believing that it represents —approximately at all events—the relative degree of risk attaching to each process, absorption being the least dangerous, expulsion the most so. Were the cases to be arranged according to frequency, the order would be somewhat different. *First* and most frequent would be the cases of sloughing; then the cases of detachment; after them the cases of expulsion; next those of calcareous degeneration; and lastly, those of interstitial absorption. I do not pretend to affirm that this order is strictly correct, for it is not deduced from any minute statistical calculation; still I think it will be found pretty near to the truth.

1. The *absorption* of a solid uterine tumour, even with the aid of medicine and other agencies likely to favour the process, is considered by some an impossibility. It is true, as a general rule, that heterologous formations are not removable by absorption. But is it correct to regard fibrous tumour of the uterus as such? Upon this point I can cite no higher authority than that of Virchow:—"We find for example," he says, "that the so extremely common form of uterine tumour, which has been designated fibrous or fibroid, has in every respect the same structure that the walls of the 'hypertrophied' uterus have, inasmuch as it consists not only of fibrous connective tissue and vessels, but also of muscular fibre cells. The tumour may, as is well known, become so large as not only to embarrass the uterus in all its functions in an extreme degree, but also to exercise, through pressure, the most injurious influence upon the neighbouring parts. In spite of this, it must always be considered an homologous structure."—*Cellular Pathology*, p. 443.

But again, in cases where a fibrous tumour (or what was supposed to be such) was absorbed, the correctness of the diagnosis has been disputed, and it is urged that the tumour was only an inflammatory or œdematous swelling, or a chronic hypertrophy, or some tumefaction whose removal we know to be quite probable. Objections of this kind may no doubt be advanced, and the possibility of an error of diagnosis in any particular instance, can very seldom be completely excluded. Still there remains an amount of evidence on this question, from which we

cannot escape drawing the conclusion, that nature is capable, on some rare occasions, of entirely removing a solid—it is to be presumed a fibrous—growth by the process of interstitial absorption. This position derives corroboration from the cases—and they are not a few—in which the same result was brought about by the use of therapeutic agents. One such case occurred to myself, and has been published. I do not here refer merely to cases—common enough—in which the tumour undergoes an apparent reduction of its bulk after treatment, menstruation, or delivery, and which reduction is to be explained by the removal of congestion or œdema. I am speaking now of the actual absorption of the growth itself.

A very striking example of the complete removal of a fibrous tumour by absorption is recorded by Dr. Matthews Duncan, and what enhances the value of this case (besides the thorough competency of the observer) is Dr. Duncan's own acknowledgment that he has been, as it were, forced against his judgment, by the evidence of a single case, to admit the possibility of the complete removal of a large fibrous tumour by absorption. He writes:—"The tumour was as large as the fœtal head at the end of pregnancy. It was as easily and as perfectly diagnosed as any case could be. There was no doubt ever thrown upon the nature of the case by any of the experienced practitioners who examined it. It had every character and symptom of a fibrous tumour. The patient was long in the most aggravated state of anæmia. *Now*, there is as certainly no uterine tumour, as there was certainly one formerly. The only method of escape for me, in the evidence of this case in favour of complete absorption, is the supposition that the tumour may have become spontaneously enucleated, separated and discharged, without the consciousness of the patient. This alternative, I confess, considering the cleanly habits and truthful character of my patient, seems more unlikely than the other."

Dr. Routh tells us in his Lettsomian Lectures that he "has met with at least two distinct cases of large fibroid, which, he should say, filled the pelvis, and materially interfered with the functions in that cavity, where the tumours have gradually diminished to the size of small apples." (p. 38.)

It is worthy of notice, and Dr. Routh pointedly alludes to the fact, that in both these instances, as well as in others, the atrophy of the tumour took place contemporaneously with the change of life, which would seem to imply that it resulted from arrested nutrition.

Whether the structure of the tumour undergoes any degree of softening preparatory to its removal by the absorbents, it is impossible to say. Dr. West thinks that it does, and a case recorded by the late Dr. Rigby rather supports this view, as he

states, in the course of its history, that the tumour became *softer* and smaller.

The processes of transformation and absorption going forward with such activity in the uterus after parturition may, perhaps, be extended to a growth standing in close vital relation to the organ, and thus lead to its partial or complete dispersion. Although parturition is likely to produce in the tumour other effects than its simple absorption, still experience shows that this latter is an occasional consequence, and the most fortunate one for the patient. "If," says Scanzoni, "we consider the great vascularity, the hyperæmia, the infiltration, and the softening which these tumours present during gestation, it will be seen that if there is any time at which the conditions are favourable to absorption it surely is the puerperal state" (p. 237 of American edition). The same high authority reports a case where "a fibrous body of the size of a man's hand, the diagnosis of which was perfectly sure, disappeared during confinement, in a manner so complete that six weeks after parturition we could no longer discern a trace of this tumour, which had existed for eleven years." (*op. cit.*)

2. *Detachment.*—From the meaning of the term detachment (or severance), it is obvious that only such tumours as are pediculated—polypi in fact—can be the subjects of this mode of elimination; but it matters not whether they spring from the mucous or the serous surface of the womb. There are three ways in which this separation may be effected, and numerous examples of each could be adduced. First, the pedicle may give way from simple atrophy and attenuation; second, when the pedicle is small it may break or snap asunder in the act of extrusion from the uterine cavity, or from the weight of the suspended tumour; and lastly, the pedicle may be destroyed through the constriction exerted upon it by the os uteri or ostium vaginæ. If the detached tumour remain in the genital canal, it will, of course, speedily decompose and become putrid.

3. *Calcification.*—A much rarer event than either of those we have been considering, is the transformation of a fibrous tumour into a calcareous mass which ceases to grow, and may be partly or entirely discharged. It is a degeneration from animal to inorganic substance. The propriety of regarding this change as a mode of elimination may be disputed. The tumour certainly remains in most cases, but it ceases to hold a vital connexion with the economy, and the symptoms which it had previously produced abate or altogether disappear.

In a very few instances the structure of these calcified or petrified fibroids has a near resemblance to bone, but such cases are quite exceptional. In the vast majority no true bone is formed, but earthy particles, chiefly salts of lime, "are deposited

and accumulated in the organic basis of the tumour, so as to make a mass which is sometimes friable and porous, sometimes hard and dense as marble" (Simon). By what process this remarkable conversion or transformation is brought about we cannot tell. Inflammatory action would seem to have no part in it. The appearance of the tumour and of its nidus indicates this. Nay, more, guided by the analogy of the cretaceous transformation of tubercle, and of the so-called ossification of arteries in the aged, we should rather regard it as the result of defective nutrition. "This much at least can, with certainty, be affirmed (writes Virchow, p. 365), that we are as yet acquainted with no stage in these changes which is at all akin to inflammation." Although we must confess our ignorance of the precise way or mode by which this calcification, or, to use the term proposed by Virchow, "petrifaction," of a fibroid is produced, still experience has shown us the circumstances and conditions which favour its production. It is chiefly met with in women advanced in life. It is confined nearly altogether to subperitoneal, or extramural fibroids; and generally to such as are pediculated, but not invariably so, as Dr. Turner would lead us to suppose. In the Museum of the Royal College of Surgeons (Ireland) is a very fine preparation, presented by the late Sir Philip Crampton, which strikingly exemplifies these points. The patient was an elderly woman, who married late in life, and never bore children. The disease was supposed during life to be ovarian dropsy, and did not appear to be the cause of death. The uterus contains several fibroids, the largest which is about the size of a fœtal head, and has the hardness and density of bone. This tumour lies almost immediately under the peritoneum, but is still imbedded in the uterus, and not in the slightest degree pediculated. In direct apposition with it is a large interstitial fibrous tumour, which contains merely a trace of calcareous deposit.

Again, we find that this process of petrifaction may be limited to the surface of the tumour, or pervade its structure. It "has no necessary connexion with the size of the tumour, and is not commensurate with its growth; but it appears (says Dr. Turner) to be in intimate relation to the size of the peduncle, and to the changes which take place, by obliteration or atrophy, of the vessels which pass from the substance of the uterus through the peduncle to the tumour."

A tumour that has undergone this calcareous transformation ceases to be productive of symptoms, except such as may arise from its mechanical influence. On rare occasions the calcified tumour has fallen from its nidus, and been discharged *per vaginam*.

It does not always prove quite so innocuous, however. Cases

have been reported in which these petrifactions, by infringing on a neighbouring organ, have caused ulceration and other ill effects. Dr. Turner cites a case where a large calcified tumour, growing from the back of the uterus, caused death by compression and rupture of a fold of the ileum, consequent upon a fall on the pavement.

Dr. Matthews Duncan exhibited, at the Edinburgh Obstetric Society, a calcified fibrous tumour of the uterus, from a patient who had died of peritonitis, and two holes were found in the peritoneum, apparently made by the tilting upwards of two thick scales of the tumour.

Through the kindness of Dr. Fleming, I once had an opportunity of seeing a most singular—indeed, I may say, unique case, in which a calcified tumour, springing from the anterior wall of the uterus, had made its way by ulceration into the bladder, producing all the symptoms of vesical calculus in their most intensely aggravated form. Some fragments of the tumour had been detached, and were found in the cavity of the bladder.

4. *Disintegration or Sloughing* is another change that may take place in a uterine tumour, and lead to its partial or complete extirpation. This process generally gives rise to symptoms of a grave kind, and in many instances the patient has actually sunk under the wasting effects of the discharge and the constitutional irritation. Suppuration of fibrous tumours is described by some authors, but I see no necessity for making a separate category of such cases. In many of them the discharge was plainly due to softening of the tumour and irritation of the uterine cavity.

The vitality of the tumour having been destroyed—how, we shall presently inquire—it may come away *en masse*, or in broken, irregular fragments, quiet putrid, at intervals of days, weeks, or months. All this while there is little or no pain, but a constant puriform discharge of a highly offensive character, and sometimes mixed with blood, is going on. In a few instances the tumour is too dense to melt away, and too large to be expelled spontaneously. Here art must interpose, or the life of the patient will inevitably be lost. Dr. Hall Davis met with two cases in the Middlesex Hospital, which strikingly exemplify the good effects of this timely interference. Very alarming symptoms, viz., fever, delirium, vomiting, &c., were present in each case; but the lives of both patients were saved by dilating the os, and removing the putrid *debris* of the tumours. I may here venture on observing that it was the study of such cases as these that led me soon after its first announcement, to form an unfavourable opinion of the operation of gouging uterine fibroids.

5. *Expulsion.*—The fifth and last mode of elimination I term expulsion, under which title are comprehended all those cases

where uterine contractions—parturient efforts, in fact—form the leading symptom as well as the efficient cause of the separation of the tumour, which is still in vital connexion with the womb when the contractions commence. In the group of cases last considered, expulsive pains were very rarely present, and when they did occur, it was *in consequence of the death of the tumour*, which then acted as a foreign body, and excited parturient efforts on the part of the womb.

As the uterus is endowed with very great contractile power, we might, *a priori*, expect solid growths to be got rid of occasionally by the exercise of this faculty. Experience amply confirms this, but at the same time teaches us what we otherwise should not be quite so prepared for, namely, that the danger arising from this mode of elimination is greater, perhaps, than from any of the preceding.

A good many cases are reported where this species of parturition has been set up, and the tumour has thereby been partially or entirely removed. A large proportion of these cases occurred in the puerperal state, which may go far to account for the large mortality amongst them. But setting aside the puerperal cases, the danger would seem to be greater from *expulsion* than from *sloughing*. This more unfavourable result admits, perhaps, of some explanation. The expulsive process is the more acute : it is often accompanied by prolonged pains of a very severe kind ; and lastly, the violent contractions of the diseased organ must powerfully tend to excite inflammation, or other morbid action in it.—*Dublin Quarterly Journal, Feb.* 1868, *p.* 20.

102.—ON DR. ATLEE'S PRACTICE IN PHILADELPHIA, U.S., IN CASES OF FIBROUS TUMOUR OF THE UTERUS.

By T. Spencer Wells, Esq., London.

[The following is taken from an article entitled "Notes of an Autumn Holiday in America." Dr. Atlee is very celebrated for his success in ovariotomy, and is now preparing a work on the subject.]

His cases of fibroid tumour of the uterus will not be less interesting than his cases of ovariotomy. In his early practice, he had, in common with myself and others, excised large fibroid tumours, believing them to be ovarian ; but had, after more extensive experience, become convinced that they were uterine. The results, however, had not encouraged him to repeat such operations, unless the patient was in imminent danger and not to be saved otherwise. He has also become much less willing than formerly to incise these tumours by the vagina. He has had some cases in which the combined use of ergot and incision

has been followed by the death and expulsion of large tumours or of their disintegrated remnants, and by the complete recovery of the patient; but the risk of uterine phlebitis and pyæmic fever has proved to be so great, that he has become gradually less willing to interfere unless driven to do so by hemorrhage or some other condition endangering life. And he has become more hopeful of these tumours becoming indolent as age advances, and at length undergoing a process of atrophy or spontaneous cure. As I had been led to entertain similar opinions after a much smaller experience than that of Dr. Atlee, I heard this with no common interest. As an internal remedy, he gives muriate of ammonia in ten-grain doses two or three times a day, for many weeks or months consecutively; and thinks he has seen more benefit follow its use, than after iodine, bromine, mercury, or any other medicine which he had tried. He promised to try the chloride of calcium, in the doses which have appeared to me to be so useful, by leading to atheroma or calcification of the nutrient vessels of these growths.—*British Medical Journal, Jan.* 18, 1868, *p.* 49.

103.—CANCER OF THE UTERUS.

[The following is from a report on the practice of Dr. Atlee of Philadelphia, by Mr. Spencer Wells, who has been lately making a kind of medical tour in America.]

In the treatment of cancer of the uterus, both cancroid and carcinoma, Dr. Atlee has great faith in the long continued use of arsenic internally, in small doses long continued, combined with the local use of a very strong solution of iodine in glycerine. A drachm of iodine and a drachm of iodide of potassium are dissolved in two drachms of glycerine, and this is applied by a brush or cotton two or three times a week all over the cervix uteri and to any part of the growth which can be seen. I saw one case of undoubted carcinoma where the patient and her husband (a medical man) fully confirmed all that Dr. Atlee said of the remarkable improvement which had followed this treatment in her case.—*British Med. Journal, Jan.* 18, 1868, *p.* 49.

104.—ON THE DIAGNOSIS OF TUMOURS OF THE BREAST.

By Thomas Bryant, Esq., Assistant Surgeon, Guy's Hospital.

What are the symptoms by which a cancer of the breast is to be made out? Cancer is essentially of a progressive character; it never remains long in one tissue, but sooner or later involves all with which it comes in contact; and it is through this

pathological peculiarity that it produces symptoms in the skin over it, and parts beneath it, of great diagnostic importance.

How Cancer affects the Skin.—The skin is affected slowly, but in a very characteristic way. It first becomes more or less fixed to the growth ; and, instead of rolling freely over its surface, as it invariably does in a simple tumour, it seems tied or drawn down to the parts beneath. A dimpling of the skin next makes its appearance ; this symptom passing into a complete puckering, at which stage of the disease the integument will have become completely fixed and immoveable. As the disease progresses, the skin becomes infiltrated with the cancerous products, and is made a part of the diseased mass ; and, as time progresses, so-called ulceration of the integument may make its appearance —this ulcerating surface always assuming a characteristic aspect. In some cases, the skin becomes infiltrated with isolated tubercles, varying in size from that of a millet-seed to tubercles of larger dimensions. When any of these conditions of skin exist with a tumour infiltrating the mammary gland wholly or in part, and more particularly the latter or tubercular form of infiltration, the cancerous nature of the disease is tolerably clear.

On the Mobility or Immobility of the Tumour.—As cancer affects the skin by infiltration, so practically it affects the parts beneath ; and, as a consequence, we find symptoms produced of great value in a diagnostic point of view ; for a cancerous tumour, instead of moving freely over the pectoral muscle, as it does in innocent affections and in health, soon becomes, as the disease progresses, somewhat fixed and at last immoveable ; the breast gland, pectoral muscle, and probably skin, all becoming, by general infiltration, involved in the same destructive influence at the last stage of the disease.

Infiltration of the Absorbent Glands.—Should you find enlargement and induration of the axillary or clavicular glands associated with a doubtful tumour of the breast, you must regard such symptoms as strongly indicative of the disease being cancerous ; for cancers rarely exist any lengthened period without causing such symptoms. It is true, that you do find glandular enlargements associated with inflammatory affections of the mamma ; but, under such circumstances, the axillary glands are usually more tender on manipulation than they are in cancer, and the enlargement is more rapid ; the cancerous infiltration, as a rule, appearing, in its early stage, as a painless chronic infiltration.

Neuralgic Pains down the Arms, &c.—This symptom is one of great value in determining the nature of a tumour of the breast; for it is rarely present in any other affection of that organ than the cancerous. It indicates the probability that the deep cervical or axillary glands are infiltrated with the disease, and

are pressing on the nerves, causing pain, although these glands may not be felt. Indeed, neuralgic pains in any part of the body, associated with cancerous disease, or after its removal, should be always regarded with suspicion; for they are often indications of secondary cancerous deposit having taken place either in the nervous centres or along the nerve's course.

Retraction of the Nipple.—I must again warn you not to place much confidence in this symptom as pathognomonic of cancer; for I have already told you that it frequently exists in other affections of the breast. It is a purely mechanical symptom, and rather reveals the manner in which the gland is involved in the disease, than the character of the affection; this opinion receiving support from the fact that the nipple may become retracted in the early stage of cancerous disease of the breast, and at a later stage reassume its prominent position.

Discharge from the Nipple.—In all cases of general disease of the breast-gland, inflammatory, cystic, or cancerous, there may be a discharge from the nipple; in the inflammatory, it may be watery or purulent; in the cancerous, it is generally sanguineous, and little in quantity; in the true cystic, it is viscid, more or less blood-stained, but profuse. It does not exist in all cases of cancer, and is not typical of the disease. As a means of diagnosis in the true cystic disease, it is of great importance.

Cachexia.—I have no belief in the existence of a special cancerous cachexia. A cachexia may be present in cancer, as in any exhausting and wasting disease; but that of cancer differs in no single point from that of any other affection. When a cachexia exists, it indicates the presence of some affection which is undermining the patient's strength; it may be cancer, but it may be any other form of disease.

Cystic Tumours of the Breast.—Time will not allow me to enter fully into this important subject. You must rest satisfied, therefore, with the few remarks I feel bound to make. I must beg you to remember that cysts are accidental developments in the majority of tumours of the breast; that they may exist in the simple, as well as in the cancerous affections; and that, as a general rule, they may be disregarded in a diagnostic point of view—the diagnosis of tumours containing cysts, with one single exception, resting upon the same points as the diagnosis of tumours without cysts, to which your attention has been already drawn. That exception refers, however, to *genuine cystic disease of the breast-gland*—a disease of the gland-ducts. It is one of an innocent nature; it affects the patient solely through local influences. It is to be recognised by the gradual and generally painless growth of the tumour; by the evidently cystic nature of the disease from the beginning; by the frequent presence of several cystic centres in the same gland; by the healthy charac-

ter of the integument over the tumour; by its free mobility upon the parts beneath; by the total absence of all indications of induration of the absorbent glands, and the general good health of the patient from the first; but, above all, it is characterised by the fact that a *free* discharge of a clear or blood-stained viscid secretion from the nipple is always present, and that this discharge can be readily increased by steady pressure upon the part; the cyst or cysts in many instances being completely emptied by these means. When the cysts are full, great pain is often experienced; when empty, the tumour is comparatively painless; this discharge of a viscid fluid from the nipple in such cases fairly determining the nature of the disease.

I have thus, gentlemen, gone over as carefully as time will allow the principal points by which the diagnosis of a case of tumour of the breast is to be determined; and I trust I have convinced you that the majority of the difficulties generally experienced in the diagnosis of such cases will be found to vanish by making a systematic use of our eyes, our ears, and hands, in the investigation of the subject. In certain cases, it is true, difficulties may be experienced; but, in the majority, a fair degree of certainty is to be attained by the adoption of such a systematic mode of investigation as I have explained and illustrated.

By way of a general summary of the whole subject, the following conclusions may be drawn up, contrasting the cancerous with the innocent affection.

1. *Cancer* generally attacks patients during the functional decline of the mammary gland—that is, after forty years of age; but *innocent* affections, as a rule, occur during the functional activity of the gland's life.

2. A *cancer* in the breast of a single woman is generally observed at an earlier age than in that of a married woman, the functional activity of the gland lasting longer in the latter case.

3. *Cancer* always infiltrates the gland, wholly or in part; but simple tumours, as a rule, seldom involve it to any great extent, with the exception of the true cystic disease.

4. *Cancer* generally affects the breast in a single centre, and from that rapidly developes. Innocent growths, cystic or otherwise, often appear as multiple tumours in connexion with one or both breasts.

5. *Cancer* generally runs its course within a period of three years; but *innocent* tumours may be developing for many years without causing much distress, affecting the patient only from local causes.

6. *Cancer* does not exist for any lengthened period without involving the skin by infiltration or ulceration; but, in *simple*

tumours of the breast, the skin remains uninvolved to a late period of the disease—until, indeed, it is ruptured by over-distension.

7. *Cancerous tumours* soon become more or less fixed to the parts beneath, and immoveable; but *simple tumours* are, with rare exceptions, moveable throughout the whole period of their existence.

8. In *cancers*, the axillary absorbent glands become indurated at an early period of the disease; in *innocent tumours*, they are rarely affected.

9. Neuralgic pains down the side and arm are common in *cancer;* in innocent affections, they are rarely present.

10. Retraction of the nipple is found in both simple and malignant diseases of the breast, and consequently is of slight diagnostic value.

11. A discharge from the nipple exists in all affections of the true breast-gland. In *cancer*, it is sanguineous and scanty; in *true cystic disease*, it is viscid and abundant, and can be readily increased by pressure on the cysts; in *inflammatory*, it is watery or purulent.

12. In cases of tumour of the breast, when the skin has ruptured or become ulcerated, the margin of the opening or sore reveals the character of the disease; in *simple* cases, presenting the appearance of being ruptured, cut out, or punched; whilst in *cancerous* diseases there is no such distinct margin, the edges of the wound appearing thickened, indurated, and everted.—*British Medical Journal*, *Nov.* 9, 1867, *p.* 417.

105.—REMOVAL OF THE BREAST—EMPLOYMENT OF TORSION.

By Edward Hamilton, Esq., one of the Surgeons to Steevens' Hospital, Dublin.

K. B., married, aged 34, has had two children; presented a florid complexion, and very healthy aspect; she complained of a tumour in the right breast of three months duration; the nipple was retracted, hard, and wart-like; the surface of the skin for some distance was thickened, and of a dusky hue; she complained of occasional lancinating pain; there was no enlargement of the axillary glands; the case thus seemed a most favourable one for operation, which was accordingly performed in the usual way, removing a large segment of skin; the vessels, which were numerous and large, were caught and twisted with Syme's torsion forceps, an instrument which I had an opportunity of seeing, through the kindness of Professor Tyrrell, of the Mater Misericordiæ Hospital. In all the operations recently

performed at Steevens' Hospital, this mode of arresting bleeding has been exclusively adopted; even in a case of amputation of the thigh last week, by Mr. Colles, the femoral trunk was the only vessel secured by ligature, all the others were sealed in the most expeditious and efficient manner by torsion, and in no single instance has there been the least appearance of secondary hemorrhage. No pocket-case should be without this instrument, as almost superseding the ligature in the treatment of bleeding from recent wounds. The wound was sponged with chloride of zinc, after the plan of Mr. De Morgan, using a weak solution, a practice which my experience leads me to regard most favourably: it arrests the weeping from small blood-vessels, cleanses the wound from clots. and produces an instantaneous glaze, very conducive to immediate union; the tumour, on examination, presented all the characters of scirrhus cancer.—*Medical Press and Circular, April* 1, 1868, *p.* 280.

106.—ON FÆCAL FISTULA FOLLOWING OVARIOTOMY.

By T. Spencer Wells, Esq., Surgeon in Ordinary to Her Majesty's Household.

[The patient, whose case is related in the following article, was 57 years of age. She had been tapped three times, and had suffered from several attacks of circumscribed peritonitis. A large multilocular cyst of the left ovary was removed on the 10th of March, 1864. The pedicle was transfixed, each half tied separately, the whole enveloped by a third ligature, and the tied end, after separation of the tumour, was returned into the abdomen with the ligatures, the ends of which were cut off close to the knots.]

A portion of the cyst adhered so firmly in the left iliac fossa that it could not be separated, and it was left adherent, after transfixing and tying it, leaving the ends of the ligatures hanging out of the lower angle of the wound. The patient recovered, and went to Leeds on the 14th of April, five weeks after the operation, the ligatures still keeping the lower part of the wound open, and a little discharge daily escaping beside them.

[After some little trouble the ligature came away, the discharge gradually lessened, and the patient considered herself to be well. There was a very slight oozing of pus, however, from the lower part of the wound.]

She remained well during the winter and early spring, but in May, 1865, Mr. Teale wrote to tell me that for some weeks past there had been "at intervals a considerable increase of discharge from the sinus, attended with uneasiness, but not with severe pain. The odour of the discharge is offensive—not putrid, but

faint or albuminous. I do not think there is any lodgment of matter. It seems to escape freely as it is secreted. Deep in the left iliac region is a general state of solidity of the parts, as contrasted with the opposite side."

It should be remembered that although the ligature which had been left hanging out through the wound in the abdominal wall had come away in May, 1864, there was no proof that the ligatures tied on the pedicle after transfixion, and cut off short, had come away. Mr. Teale thought they might be present, and keeping up irritation. He adds, "To-day I examined the sinus with an elastic catheter, and at the depth of 4½ inches encountered a solid resistance. Having introduced a hollow elastic tube open at the end, I passed through it a fine wire stilet, hooked at the end, and tried to angle for the retained ligatures, but without success. These proceedings were conducted in the most gentle manner and did not cause the least distress." On the 25th of May, Mr. Teale again wrote "she has been slightly feverish and frequently troubled with diarrhœa, and I have not thought it right to attempt any mechanical treatment. The discharge is less in quantity, but I think it has shown a little fæcal tinge."

After this the discharge became more abundant and more decidedly fæcal, varying in quantity from day to day. She often complained of a feeling of painful distension at the lower part of the abdomen. This was generally followed by a gush of acrid irritating discharge, and then by relief. The amount of fæcal matter in the discharge varied considerably. If the bowels were not relaxed, there was little or none. Latterly, however, as she became confined to bed, she had frequent attacks of diarrhœa, and then fluid fæces escaped in considerable quantity from the fistula. But no solid fæces ever passed. She gradually became weaker, and she died Dec. 20, 1865, about 20 months after ovariotomy.

I am indebted to Mr. T. P. Teale, junr., for a report of the *post-mortem* examination. "The fistulous opening on the surface of the abdomen was large enough to admit the tip of the little finger. Within the abdomen it was so dilated as to admit a middle finger at least. On opening the abdomen we found the edge of the omentum adherent to the wall at the level of the wound,—a coil of small intestines sealing the wound above the fistula, which latter was at the lower extremity of the wound. The omentum and sub-peritoneal tissues were excessively loaded with adipose tissues. A small part of the small intestine, the sigmoid flexure, and the rectum were matted together around the fistula and the left corner of the uterus. Close to the left side of the uterus was a mass, almost spongy, and pediculated, which projected towards the rectum. In the centre of

the mass was a large suppurating cavity which communicated with the fistula, and with the rectum, by two large openings. The cavity extended for some distance between the uterus and the rectum. It passed towards the right side behind the lower part of the uterus ; downwards, by the side of the rectum ; and forwards as far as the femoral ring. No trace of any ligature could be found. The right ovary was healthy. The liver was greatly enlarged and much altered by fatty degeneration."

I think this case tells against the practice of tying the pedicle of an ovarian tumour, whether the ends of the ligature are left hanging out between the lips of the wound, or are cut off short and returned. The formation of a sort of canal or sinus by the adhesion together of folds of omentum or coils of intestine, in such a manner as to enclose the ligature and shut it off from the general peritoneal cavity, occurs, I believe, very generally when the ends of the ligature are not cut off. If the patient recover, one might expect more or less obstruction of intestine to follow such adhesions ; although I am not aware of any case where such obstruction has been actually proved. When the ends of the ligature are cut off and the pedicle returned, we know from repeated *post-mortem* examinations that a similar adhesion of neighbouring intestine takes place around the end of the pedicle ; and that, in some cases, pus has been circumscribed in this manner—until at length it has found an outlet, either through the abdominal wall, the vagina, or intestine. The occasional observation of cases of this kind (when at distant intervals I have been driven to adopt one or other of the *intra*-peritoneal methods of dealing with the pedicle) has led me more and more to the conviction that the clamp, or some other *extra*-peritoneal method, is not only more successful as regards the immediate result of the operation, but even more so if we look to the subsequent health of the patient.

Patients who recover after the *extra*-peritoneal treatment of the pedicle, as a rule, soon regain and maintain perfect health. So do many of those who recover after the *intra*-peritoneal treatment. But some of them, sooner or later, suffer from chronic suppuration, hæmatocele, or fæcal fistula ; or, perhaps without any definite local ailment, are many months before they become strong and well.—*Glasgow Medical Journal, Feb.* 1868, *p.* 379.

107.—A NOTE ON MENORRHAGIA.

Case under the care of Dr. MURRAY at the Great Northern Hospital, London.

Troublesome cases of menorrhagia treated with success are always worthy of record, for there are few disorders more trying

to the medical practitioner. The result of treatment in the following instance, which was lately under Dr. Murray's care at this hospital, was very favourable. The patient was a woman of highly scrofulous habit. She had had many children, and her uterus was large, heavy, and indurated, with the mucous membrane of its interior granular, and bleeding at the least touch. The loss of blood was severe at the periods, and continued during their intervals, though not to so great an extent. She had, however, sustained so much loss, that complete amaurosis had occurred, and her health was otherwise most seriously damaged,—the nervous system being generally shattered.

Dr. Murray, after having tried plugging, and the various styptics usually employed, even injecting the uterus with a solution of iron, met with great success in adopting the course which was originally, we believe, suggested by Dr. Henry Bennett,—namely, plugging the uterus itself.

Three or four small pieces of cotton wool were used, to each of which were fastened long pieces of silk for the purpose of withdrawal. These little plugs having been first dipped in a solution of the sesquichloride of iron, were carefully pushed through the speculum into the uterine cavity, by means of the sound, and allowed to remain *in situ* from twenty-four to forty-eight hours, the iron preventing any unpleasant consequences from decomposition. This treatment was pursued for about ten days, after which time menstruation took place in a normal manner, and there followed no hemorrhage of any kind.

Dr. Murray is disposed to think that the delay which often takes place before local remedies are resorted to in some cases of menorrhagia, is frequently attended with the most mischievous results. The timely application of various forms of styptics to the uterine membrane will often save the patient from the serious and sometimes irreparable consequences of a long-continued drain from the system.—*Lancet, Jan.* 4, 1868, *p.* 12.

ADDENDA.

108.—ON THE THERAPEUTIC ACTION OF ACONITE IN DILATED CONDITIONS OF THE BLOOD-VESSELS.

By Dr. Archibald Reith, Physician to the Royal Infirmary of Aberdeen.

[In accordance with the experiments of physiologists, the vaso-motor system, when paralysed, is more susceptible of stimuli than in the natural condition, and in consequence only very small doses of medicines having an affinity for the affected organ are required to contract unduly dilated blood-vessels.]

One of the first medicines on which I experimented was *Aconite*, to the therapeutical application of which, in accordance with the foregoing theory, I shall devote this paper. I do not intend to touch upon all the remedial uses of this drug, but merely to show its great value in one morbid condition of the system, namely, the febrile state. Before doing so, however, it will be proper to notice some of the more prominent characteristics of aconite, as recognised by the latest authorities who have made it the subject of their observations.

In my last paper I stated that medicines, viewed in relation to their effect on the blood-vessels, might be divided into two classes—the first consisting of those which merely induce contraction without subsequent dilatation, the pure astringents, acting chemically on the tissues; and the second, much the larger class, comprising those whose primary action of contraction (stimulus of sympathetic) is followed, sooner or later, by a corresponding dilatation of the vessels (paralysis of sympathetic). Passing by the former for the present, we find that the second class, those possessing the afore-mentioned double property, may be subdivided into two sections :—1. The medicines whose primary stimulating action on the sympathetic (contracted vessels) is prolonged and decided, and whose influence must be partially or wholly removed before re-action takes place (dilated vessels and paralysis of sympathetic); 2. The medicines whose primary action of spasm is momentary, scarcely, if at all, perceptible, though intense, and is followed, during the continuance of their influence, by very manifest and decided dilatation of the blood-vessels (paralysis of sympathetic). Cold is an example or type of the one class, heat of the other.

Perhaps the greater number of drugs belong to the latter class, manifesting considerable variety in the intensity and duration of their temporary stimulant action on the sympathetic, but resembling each other in the shortness and comparative imperceptibility of the stimulus. Aconite, on the other hand, is an example of the former class of medicines. The analogy between its poisonous action on the system and the effects of cold are very striking. The prolonged contraction of blood-vessels with the numbness characteristic of cold, when applied to the body, have their counterpart in the phenomena exhibited by aconite. Hence these two agents have hitherto occupied a place in the Materia Medica different from most other remedies. I stated formerly that what were known as the physiological or toxic properties of medicines were in reality their secondary effects, being a reaction from a previous, often invisible, or, at least, unobserved state of spasm of the blood-vessels. In experimenting with drugs, observers had overlooked this primary condition, and had wholly concentrated their investigations upon the secondary condition of dilatation. The majority of agents belonging to the second class, mentioned a little time ago, their primary action is comparatively of short duration, and it is not surprising that it should have escaped attention. With aconite and cold the case is reversed. The primary effect of these agents, that of contracting the blood-vessels and exciting the sympathetic, is so decided and prolonged that it has constituted their main feature in descriptions of their properties. Thus the recognised physiological properties of cold and aconite are due to their primary action, those of other remedies to their secondary. It is easy to see what confusion this has introduced into therapeutics. The double action of medicines has been entirely disregarded ; in some the primary, in others, such as aconite, the secondary phenomena have been overlooked. Both these actions, however, must be taken into account before we can form a correct estimate of the therapeutical value or application of remedial agents.

The chief brunt of aconite falls on the nerves of the heart. When taken internally, although other organs of the body come under its influence, its main action is upon the centre of circulation. If we could suppose Dr. Richardson's ether spray directed on the heart, it would give a fair idea of the effect of aconite upon that organ. Its mode of operation seems to be to cause excitement of the cardiac plexus with consequent contraction of the blood-vessels supplying the tissue of the heart. There is a diminution of blood in, and a decrease of the vital properties of, the organ. Its pulsations become less frequent, and may fall so low as forty per minute. The surface of the body becomes cold ; there are rigors, shiverings, cramps, denoting

defective circulation in the system. The general condition of the patient in this state resembles the collapse of cholera. In fact, it is a true collapse, induced by a poison, as much as is that of cholera. The sympathetic system is in a state of excitement, and the blood-vessels in a state of contraction. If a moderate dose of the poison be taken, the symptoms induced are those of the chilly stage of a fever, a less advanced degree of collapse. These, then, are the recognised physiological effects of aconite on the circulation. The attention of observers seems to have been arrested at this stage. Any further phenomena, characteristic of the medicine, appear to have been totally overlooked, or, at least, considered unworthy of special notice. Whereas the truth is, that the subsequent manifestations of aconite on the circulation are equally important with, if not more so than, those just mentioned. In all cases of collapse, unless the influence be strong enough to extinguish life in that stage, there is of necessity a reaction to the opposite condition. So much is this recognised, that it is the practice in India to abstain from all interference with cases of cholera during the stage of collapse, and to wait for the reaction which is certain to occur in the majority of the patients. The chilly stage, also, of a fever is succeeded by the febrile stage. In like manner the primary symptoms of aconite just detailed are followed by a corresponding reaction. In proportion to the stimulus exerted upon the cardiac nerves, more or less paralysis ensues. The blood-vessels of the heart are preternaturally dilated, more blood is sent to its tissue, and there is a consequent increase in its vital properties. The pulsations are increased in frequency and strength, the circulation is more active, and there is consequently a febrile condition present. Had our nomenclature been consistent, this aconite-fever should have been regarded as the physiological action of the drug, and brought it under the same category with other medicines whose secondary symptoms have been considered their proper physiological manifestation. If the duplicate phenomena, however, be admitted, the nomenclature is of little importance. Now, these febrile symptoms exhibited by aconite, though acknowledged by the profession, are nevertheless practically ignored. They are never mentioned as affording any indication of its therapeutical application. All our ideas of aconite have hitherto been confined to its primary action as much as they have been to the secondary action of other medicines. But, in possessing a double property, its use in disease is rendered more intelligible and accurate.

It is unnecessary, I think, to enter more minutely into the influence of aconite on the circulatory system. Its action is so well known, that any detailed account would be superfluous. As I intend to confine my remarks to its remedial uses in the

febrile state, it is equally unnecessary for me to enter upon its effects on other parts of the economy.

The secondary action of aconite being attended with dilatation of the cardiac blood-vessels and quickened circulation, it follows that, in accordance with the views formerly expressed, it should prove more or less remedial in conditions of the system where such dilatation existed—the dose, of course, being small enough to induce only the primary effects. The febrile state is that for which aconite is specially indicated. And herein is exemplified the advantage of recognising the double properties of medicines. Aconite, viewed solely as a depressor of the heart's action, that is to say, a stimulant of the cardiac plexus, has been strongly recommended by some physicians in various kinds of febrile disorder. But overlooking the secondary action of the drug, and guided only by its primary, they have prescribed it on the principle of the larger the dose the more powerful the effect. The experience of its use in this way is very diverse. Sometimes it has succeeded; but it has as often failed, and in many cases dangerous symptoms have occurred. Sir James Simpson says, "One medicine has of late been supposed to produce this sedative effect on the heart, without exerting any other appreciable therapeutic action. I allude to aconite, a strong tincture of which has been employed by various European surgeons in surgical fever; but it is difficult to regulate it in its effects, and in many persons it produces an almost dangerous and depressing effect, even in very small doses." Aconite has therefore not come into general use as a febrifuge. But when the principles are applied, which I endeavoured to expound in my last paper, we obtain the true conception of its therapeutical application. The two following points may be noticed:—

1. In the first place, the secondary action of aconite being analogous to the febrile state, we infer that it will prove remedial in cases of simple uncomplicated fever only.

2. In the second place, the effect desired being to produce moderate contraction of the cardiac blood-vessels such as will not be followed by undue reaction, the dose of aconite must be correspondingly small; and on account of the extreme susceptibility of the paralyzed sympathetic to stimuli, care must be taken to regulate the dose accordingly.

The first proposition excludes aconite almost entirely from the domain of specific fevers (except, perhaps, rheumatic fever, of which I shall speak presently). I have used it extensively in typhus and enteric fevers, without the least advantage. Sometimes I found the pulse to come down a little, but it invariably rose afterwards. In the majority of instances no impression whatever was produced on it. It is therefore vain to expect any good from the employment of aconite in such cases. I should

think, also, that in other specific fevers, such as measles, scarlatina, small-pox, or erysipelas, the same negative results would be obtained; but I have not tried it in these cases. Mr. Liston spoke highly of it in the last-named disease, but his example does not seem to have been followed. Belladonna is so beneficial that I have not resorted to aconite when treating erysipelas. My experience of the latter medicine in the two continued fevers I have mentioned is decidedly unfavourable, and confirms the impression I had formed from its secondary action, that it would be useless in fevers depending on the presence of a specific poison in the blood. Then, again, even in inflammatory fever, for which it has been highly extolled, and with reason, no impression will be produced on the symptoms *if there be extensive local lesion.* In such a case, the medicine acting physiologically on the affected part is the remedy properly indicated, whether it be belladonna for meningitis, antimony for pneumonia, or cantharides for nephritis. Aconite may and does prove very serviceable in conjunction with these, but by itself it will probably fail. Its use must therefore be limited to simple fever without local complication.

Next, as to the dose. There can be no doubt that the conflicting testimony regarding aconite is in great measure due to the quantity prescribed. Forgetting the febrile reaction which takes place, the tendency among observers has been to use gradually increasing doses. Finding injurious effects produced, they went backwards; only, however, to a certain extent. In some cases even small doses were found capable of acting injuriously. The medicine was consequently abandoned altogether. It never seems to have struck any one that, by still further diminishing the dose, more accurate results would have been obtained. On the contrary, the idea has been taken up, as with many other medicines, that unless benefit were derived from the doses of the Pharmacopœia, it was contra-indicated. The old-fashioned phrase of "not agreeing with the patient," became the usual house of refuge. Now, I beg to call attention to the following fundamental axiom of therapeutics, which has never, so far as I am aware, been stated before. It is this:—*Medicines, whose recognised physiological action is in reality their secondary manifestation, when indicated by the pathology as proper remedies for a particular disease, are unsuitable for, and injurious in that disease, when given so as to develop the physiological symptoms.* We have a notorious example of this in quinine, which "disagrees" with so many people. The disagreement is merely the development of its secondary or physiological action. If it be the proper remedy for the patient's ailment, the dose ought to be reduced. If one grain disagree, less and less ought to be given till the proper quantity be found. Many a case of this

kind have I met with, and many a case of dyspepsia have I cured with that very quinine which the patient fancied he could not take. What is called idiosyncrasy is, in nine cases out of ten, merely greater susceptibility to the action of medicines; and if the dose were regulated in accordance with the foregoing principles, these peculiar abnormities would be less frequent. In the case of aconite, the physiological action being primary, we are merely called upon to deal with an excess of that action as manifested in the experience of those who have used it. They find that, in febrile cases, even five minims of our tincture, or three minims of Fleming's, will often cause alarming symptoms. Such a dose would have little effect on a healthy subject. But in conformity with that morbid susceptibility which I have repeatedly insisted on, in the febrile condition it becomes excessive. Why, then, not diminish it? This is the very point where experimenters have failed. It seems singular that when they found five minims to depress the heart too much, they did not try four, three, two, or even one. If they had done so, the value of aconite would have been by this time universally acknowledged.

The preparation I used in my investigations was the common tincture of the British Pharmacopœia. Keeping in view the theory on which I was experimenting, I gave much less doses than the minimum of the prescribed standard. I found then, that to answer all the purposes for which aconite was indicated, it was not necessary to give more than one drop, nor less than a quarter of a drop. On three occasions I found even this latter quantity to act too powerfully. It may be satisfactory to give these cases, the symptoms being alike in each.

A gentleman, aged 30, who had a feverish catarrh, took, according to my prescription, one quarter of a drop of tinct. of aconite, in water. In a few minutes faintness came on, the pulse became very feeble, and there was a feeling of loss of power in the left arm. Brandy was administered, and recovery gradually took place. This patient is liable to feverish colds, but he never takes more than one-eighth of a drop of aconite, which he finds invariably to act as a specific.

A lady, aged 28, in similar circumstances, took a quarter of a drop of the tincture. Soon afterwards her pulse fell very low, and this symptom was attended with powerlessness in the left arm.

A lady, aged 31, suffering from febrile catarrh, took tinct. aconiti gtt. $\frac{1}{4}$. In a few minutes similar symptoms came on as in the other two cases. On two succeeding occasions I prescribed the same quantity of aconite, with the same results.

With these exceptions, then, the remedial dose of aconite I have found to lie between the quarter and the full drop. It

may be given in water simply every half-hour, every hour, or every two hours, according to circumstances—always, of course, upon the empty stomach. This quantity may, at first sight, appear absurdly small, and to approximate the homœopathic infinitesimals. It is not, however, a great reduction from the minimum dose of the Pharmacopœia (5 minims), which is confessedly too energetic in many cases. There is a wide difference also between gtt. $\frac{1}{4}$ as a minimum dose, and the thousandth or millionth part of a drop given by Hahnemann, in accordance with his theory of potentization. I do not know the strength of the homœopathic preparations of aconite, nor the relation they bear to our tincture; but it is plain enough, from homœopathic publications, that the more intelligent and enlightened members of Hahnemann's school are giving up the theory of their founder, and prescribing doses more consistent with reason, although still on the principle of *similia similibus curantur*. The spread of homœopathy is due to the powerful effect of small doses of such medicines as aconite. Separate it from infinitesimals, and there is little fault to find; but an illogical public reasons in this way:—"Aconite cut short my cold, *ergo*, Homœopathy is right." The more intelligent method is certainly to reason, as a gentleman did who, having been cured of a feverish cold in a few hours by some aconite given to him by a homœopathic friend, said to me:—"It convinced me that aconite was a very powerful medicine, but it did not convince me that homœopathy was true." Now, if the middle position were adopted, which I have indicated in these papers, the present homœopathic system would not have a leg to stand upon. The doses I recommend are what come short of reaction, whether it be 20 grs. of bromide of potassium, or one-fourth of a drop of tincture of aconite. The connexion between this and infinitesimalism is certainly very slight.*

I come now briefly to indicate the applications of aconite as a remedy for febrile disorders, in so far as I have made it the subject of personal observation.

1. *Catarrhal Fever.*—We possess in aconite a medicine which quite supersedes the purgatives and antimonials with which patients are too often drenched, *usque ad nauseum*. By purgatives the dilatation of the cardiac blood-vessels, or paralysis of the cardiac plexus, is sought to be relieved, by creating dilatation

* I find that many of the enlightened and intelligent homœopathic practitioners are of the same mind. They are prepared to abandon Hahnemann's system, and to occupy a middle position such as I have indicated, did they receive encouragement from our side. Would it be too great a concession to give them that encouragement? We are fully aware that homœopathy contains an element of truth: shall we continue to reject that element merely because Hahnemann buried it in so much rubbish? I cannot believe so; and I think that the recognition of the double action of drugs might serve as a basis on which the two opposing schools could ultimately be reconciled—a consummation devoutly to be wished.

of the abdominal blood-vessels, or paralysis of the solar plexus. With antimony it is the same; acting by preference on the stomach and lungs before the heart, it lowers the pulse only at the expense of the first named organ, and after a large dose. Aconite, on the other hand, acting directly on the heart, controls its force prior to any influence exerted on other organs, and therefore, in quantity insufficient to operate elsewhere, lowers the pulse without the weakening effect of purgatives, or the nausea and unpleasant consequences of antimony. It is scarcely possible to speak of it too highly as a remedy for febrile catarrh, in the doses recommended above. For this it acts like a specific. It will not cure the attendant coryza, or tonsillitis, or bronchitis; but a few oft-repeated doses will, almost to a certainty, bring down the pulse, rendering it softer and less frequent. Its operation is usually attended with more or less perspiration, and followed by the removal of the oppression and restlessness under which the patient laboured. In order to save time I have frequently administered the medicine myself, and have, before leaving the room, witnessed the good effects of a single dose. It is quite common for me to hear parties express themselves as greatly relieved a few minutes afterwards. I have frequently had occasion to use it myself, and it has always within ten minutes removed the distressing feelings. A second supply was never required. In the majority of cases, three or four doses are sufficient. Indeed, if within twelve hours no improvement takes place, it is to be inferred that there is a specific poison at work; and hence aconite forms a diagnostic element. In not a few instances I have predicted the outset of typhus or enteric fever from the failure of aconite to bring down the pulse.

It would be impossible, within the limits of a short paper, to give even an abstract of the numerous catarrhal cases treated in this way and cured. Their number is legion. I shall therefore give only a few as examples of the whole. They do not differ materially from each other, and it would be an unnecessary repetition to detail every one.

Case 1.—A. G., female, aged 50, had suffered from general restlessness and feverishness for several days. When I saw her, the pulse was 120, the skin hot, and the tongue furred. She complained very much of "pain in her bones." I gave her at once gtt. $\frac{1}{4}$ of tincture of aconite in water. In five minutes, before I left the room, she felt very much relieved, and expressed her astonishment at the rapid effect. Before morning (it was 8 p.m. when she first took the aconite), she was quite well, and able to be up.

Case 2.—H. M., female, aged 13, was seized with rigors and general coldness on a Sunday afternoon, followed in the evening

by great feverishness, headache, and oppression. I prescribed gtt. $\frac{1}{4}$ of tinct. aconiti every two hours, to be suspended if she felt better after one or two doses. She did not require to take it more than twice, and before morning she was well.

Case 3.—A. B., male, aged 60, had smart coryza, with great restlessness and general oppression. He had been in a "burning fever" the whole of the previous night. He was ordered to take gtt. $\frac{1}{4}$ of tinct. aconiti during the night, at short intervals. A few doses relieved the symptoms, but without perspiration, and in the morning he was well.

Case 4.—I was called at midnight to see a child, aged $2\frac{1}{2}$ years, said to be attacked with croup. I found him in a strong fever, with flushed face, swelled tonsils, and great dyspnœa. The croupal symptoms had lasted for more than an hour, but the child had been feverish and unwell for several days. On the first appearance of the stridor and hoarseness the parents had very properly put the child into a warm bath, and applied a poultice to the chest. I ordered mustard to the throat, and gtt. $\frac{1}{8}$ of tinc. aconiti every hour. I was so satisfied that this was more than laryngismus stridulus, that I told the parents to call me at 7 a.m., if the child was not better. I had some misgivings about not giving the usual antimonials, and determined that, should the aconite fail, as little time should be lost as possible. I was not sent for; and when I called in the morning I found the child quite well, free of stridor, and hoarseness, and fever. He was, in fact, amusing himself with his toys. I was told that the alarming symptoms had subsided about 4 a.m. Query, Would antimony or any other medicine cut short threatened croup so effectually as aconite did in this case?

Aconite is also very useful in the chill which precedes the fever. I have met with several cases of this kind. One, a young man, sat shivering over his fire for an hour before he took the medicine. In twenty minutes the shivering ceased, as also the malaise which accompanied it. I infer that the aconite prevented the usual reaction which would probably have taken place.

Notwithstanding what I have stated, aconite does sometimes fail, even in catarrhal fever. Success, however, is the rule; want of it the exception. The reason of the exception I have not been able to make out; but it ought to be regarded, perhaps, as a confirmation.

II. *Rheumatic Fever.*—Aconite has been recommended by many for acute rheumatism, but very conflicting accounts are given regarding it. By some it is extolled highly; by others as much depreciated. Lombard of Geneva, who used it extensively, gives the following conclusions:—

1. The alcoholic extract of aconitum napellus is endowed with a specific action against acute articular rheumatism.

2. It speedily dispels the pains and the swelling, and dissipates the effusions of synovia in the articulations affected by acute rheumatism.

3. The medicine does not act as a derivative on the intestinal canal or on the skin.

4. Given in large doses, it produces a strong stimulus on the brain, and appears to modify the circulation.

5. The alcoholic extract of aconite contains the active principles of the substance, at least as regards its anti-rheumatic properties.

Fleming and Neligan in our own country have followed Lombard in their appreciation of aconite in rheumatic fever. All these observers, however, used gradually increasing doses; and this is probably the reason why, as I explained a little ago, conflicting results have been obtained, preventing the general adoption of their practice. My experience of aconite in rheumatism is very favourable. The small doses are unattended with the risks of larger ones, and produce all their good effects. I always trust to aconite for the reduction of the pulse, and it seldom fails, although it does not operate so speedily or so effectually as in catarrhal fever. For there is undoubtedly a poison in the system, and its presence there must necessarily interfere with the free play of aconite on the circulation. But the rheumatic poison does not resist aconite so completely as other febrile miasmata, and consequently the remedial effect of the medicine is far from being antagonized. While the symptoms in catarrhal fever ought to be relieved within a few hours, twenty-four at the most, I am satisfied if, in rheumatic fever, the pulse come down in two or three days from the time of administering the aconite. The utmost I expect from this is, not the instant cure of the disease, as some have hoped, but the protection of the heart from complication, and, by the quietness of the circulation, giving full advantage to the excretory organs to eliminate the morbific element. This is all that aconite will do, and it is a great deal. It prevents us being driven by the entreaties of the patient to that most mischievous drug, opium, and so saves the sufferer weeks of misery. It shortens the duration of the fever, if it does not arrest it. It is an invaluable auxiliary to the alkaline and blister treatment, and by itself alone will often save the necessity of resorting to either. The following cases will serve as illustrations:—

Case 1.—J. F., aged 21, male, affected with acute rheumatism. Pulse 120, very strong and full; knees very much swollen, especially the left; ankles painful and red. Ordered gtt. $\frac{1}{4}$ of tinct. aconiti every two hours. Next day the pulse had fallen to

80, and the swelling of the knees had almost entirely disappeared. This patient's illness lasted a week. He had been ill three days before I saw him; he was relieved of the more severe symptoms in twenty-four hours, and in two days more he was comparatively well. Cases of this kind are frequently met with, where aconite alone suffices to effect a cure.

Case 2.—E. A., female, aged 28, a servant, was admitted into the Infirmary on the 2nd of January. She had been ill for a week with rheumatic fever. When seen, all the joints of the limbs were swollen and painful, the two wrists and the right knee being most affected. Pulse 120, full and strong. No cardiac complication. She was ordered gtt. $\frac{1}{4}$ of tinct. aconiti every two hours. The pulse fell at the end of two days, and her general distress was much alleviated, but the joints, especially the wrists, continued painful. The blister treatment was then adopted for the wrist and right knee with complete relief, and she rapidly recovered. The pulse never rose after it had been reduced. This case is a good illustration of the failure of aconite by itself to cure the disease, but that it is very beneficial in conjunction with other remedies. Perhaps if it had been prescribed at the outset of the fever, it might have been successful alone.

The next case is an example of the complete inefficacy of aconite in severe local complications.

Case 3.—A. R., female, aged 18, admitted to the Infirmary on 9th of January, suffering from rheumatic fever of several days duration. She had formerly had the same disease about two years ago, and was then under the care of Dr. Smith. The heart was affected at that time. On admission the knees and ankles were found most affected, and there was a double murmur over the aortic valve, and apparently a to-and-fro sound over the apex. A few days afterwards, pericarditis was fully developed, and ran its usual course. From the first she took gtt. $\frac{1}{4}$ of tinct. aconiti every two hours, without the smallest impression on the pulse, and it was not till after smart blistering that it began to fall. This is the only case of the kind I have met with for some years, as very few of the patients suffering from rheumatic fever are attacked with cardiac complications under the improved treatment now in vogue. It was not a case where aconite might be expected to do good, and larger doses than what I gave might have proved too depressing.

In mild cases, then, of rheumatism, aconite will in most cases lower the pulse and very materially shorten the disease, without further medicinal agency. In more severe cases, while it will prove serviceable in controlling the heart's action, it will be necessary to give it in conjunction with the full alkaline or

or blister treatment. In carditis, it will probably prove ineffectual.

[In inflammatory fever aconite has no place when there is extensive local lesion. The medicine then indicated is the one acting physiologically on the affected part, such as ipecacuan and antimony in bronchitis and pneumonia. The true place of aconite is at the onset of local inflammation, or the congestive stage; it is then most beneficial. In the shock after severe labour Dr. Reith always prescribes aconite, and can testify to its great value, not only a febrifuge, but as an antagonist to the usual febrile reaction which then takes place.]—*Edinburgh Medical Journal, April* 1868, *p.* 894.

109.—ON THE PHYSIOLOGICAL ACTION AND THERAPEUTICAL USES OF CONIUM.

By Dr. HARLEY, Assistant Physician to King's College Hospital.

Physiological Action.—Hemlock acts as a depressor of the muscular movements, but the effect is influenced by the state of the muscles, whether they are in action or at rest. If a vigorous adult man take five or six fluid drachms of the succus conii and then start on a long walk, in half or three-quarters of an hour he experiences a feeling of tiredness, especially in the knees and hamstrings. He may still continue to walk, feeling powerless, with some giddiness and feeling of heaviness over his eyes; in an hour the sense of fatigue has gone off, and in another hour he is as active as ever. If five fluid drachms and a half be taken and he remain at rest, the eyes become first affected—the adjusting function is interfered with—then succeed drowsiness and dilatation of the pupils, then weakness of the legs; he becomes cold, pale, and tottering; the pulse is regular and of undiminished force and volume; there is a diminution of muscular power in every part of the muscular system, and almost paralysis of the hamstrings and levator palpebræ. These symptoms are at their maximum about two hours after taking the dose, and have quite disappeared in three hours and a half. The succus was administered to several persons in doses of from three fluid drachms to one fluid ounce with similar symptoms, but in doses short of three or four fluid drachms there is no appreciable symptom. The action is uniform and invariable in man and in all other animals. There is depression of the motor function of the third nerve, a lazy movement of the eyes, and sometimes strabismus, with imperfect adjustment of the refracting media of the eye. The effects are quite distinct from those of alcohol—the latter acts on the motor system only secondarily, and does not affect the action of conium. The full

action of hemlock is asleep; it is to the corpora striata, the smaller nervous centres, and the whole of the motor tract, just what opium is to the brain—it tranquillises and renovates the whole muscular system. At first it appears to paralyse this, but it is really a tonic, for Dr. Harley administered conium in full doses for months, and found the condition of the body was improved. The action of conium is influenced by the muscular activity more than by the muscular power. The sedentary with abundance of strength are more affected than the delicate but active. A delicate child will often take as much as would reduce some strong men to a tottering condition. Hence the dose of conium must be proportioned to the degree of motor activity of the individual, whether child or adult; in fact, it might almost be said that by conium we may measure the bodily activity of the individual. Hemlock has no pure cerebral effects; the irritability of the spinal cord is diminished; there is no evidence of distinct interference with the sensory functions.

Mode of Action on the System.—In thirteen cases in which it was administered the secretion of urine was in no way altered. By acting on the mixed urine of three cases, both before and after taking the drug, with potash, there was in both mixtures a similar smell of conia; hence it is not eliminated by the kidneys, and could not be detected in the breath or fæces. It has no direct action on the sympathetic, but nutrition is improved. Pain in cancer is diminished, probably from relaxation of muscular fibres, as the division of neighbouring muscular fibres relieves irritable ulcers. A dose of conium which falls far short of physiological effects is of no more use than a dose of two grains of quinine in ague. He attributed the neglect of conium as useless, to the administration of too small doses.

Therapeutical Uses.—There was only time to refer to its beneficial action in the convulsive diseases of children. It is pre-eminently a children's remedy. He gave it to a child $1\frac{3}{4}$ years old suffering from laryngismus stridulus and convulsive cough on excitement; he began with twenty minims, then thirty minims, forty minims, and increased it up to two fluid drachms and a half. There was no appreciable effect till he took forty or fifty minims. There was one attack in the first five weeks (previously one in two weeks), and the child completely recovered. In another case there was a complete tetanic condition of the muscles of the feet, with hyperæsthesia, drawing in of thumbs, contraction of hamstrings, &c., in a child $2\frac{3}{4}$ years old; there were tetanic attacks every now and then, lasting from five minutes to several hours. It got better under the use of conium, but, having omitted it for a time, relapsed, but on resuming it completely recovered.—*Medical Times and Gazette, March* 21, 1868, *p.* 325.

110.—ON THE ACTION AND USES OF BELLADONNA.

By Dr. J. Harley, Assistant-Physician, King's College Hospital.

[The following is an abstract of the second of the Gulstonian lectures delivered at the Royal College of Physicians.]

Dr. Harley administered physiological doses, short of toxical effects, to man, the horse, and the dog. The method usually adopted was subcutaneous injection, and the solution one of sulphate of atropia. To man this was administered in doses varying from $\frac{1}{150}$th to $\frac{1}{80}$th of a grain. The symptoms were more or less delayed and powerful according to the dose, but the sum of its action seemed to be as follows:—After a few minutes there was a rather sudden acceleration of the pulse from twenty beats to double its previous frequency: its volume and force were also augmented; the temperature was increased about 1° Fahr. externally and slightly internally, and there was heaving of the carotids, also giddiness, heaviness or somnolency, nervous delirium and startings; dryness of the tongue, palate, and throat, with huskiness of voice; a dry brown fur on the tongue, which was usually moist at the tip and edges; gradual and increasing dilatation of the pupils. In two or three hours the dryness of throat and tongue gives way, and there is a very acid viscid secretion, of a fishy odour; then the pulse falls, but the pupils are in their highest state of dilatation. There is no effect on the respiration; occasional sighs and prolonged yawns. After the pulse assumes its ordinary rate the giddiness passes off, and he seems the same as before; but he feels languid, and there is dulness of mind and diminution of vision. Headache during these symptoms or afterwards is rare and exceptional. Desire for food soon returns, but during the action of the drug insalivation or deglutition is impossible. The same accelerating effect on the pulse was observed on giving it by the alimentary canal, and with succus belladonnæ there were exactly the same symptoms, only altered in rapidity.

Conditions interfering with its Action.—1. Children are remarkably insusceptible, and occasionally bear very large doses before cerebral symptoms and dryness of throat are produced. But dilatation of pupil occurs as rapidly in the young as in the old. 2. Among adults some are more susceptible than others; it is doubtful whether pregnancy does not fortify against it. 3. Fixed caustic alkalies decompose the active principle, but only after a time; if taken immediately on mixing, its power is unaffected.

Results of Action and Mode of Elimination.—The kidneys are very active in its elimination from the minute it enters, and in two, or at most three, hours, all is removed. This occurs at all ages and in all conditions of the kidney. It is easily detected

by its action on the pupils, and its presence can be easily proved if only $\frac{1}{90}$ grain of sulphate of atropia has been given, as the urine is generally retained; this is useful in a Medico-legal point of view. During the operation of belladonna the amount of urea excreted is increased, and the amount of sulphates and phosphates still more so, while the chlorides are usually diminished. The urine is increased in quantity and specific gravity. There is no increase in the uric acid, but sometimes a diminution. Hence it will be seen that it has a very similar effect to febrile action, perhaps from the same influence upon the sympathetic system, to which it is a powerful stimulant; but if its action is continued sufficiently long, the vaso-motor nerves are paralysed, and dilatation of the vessels occurs. The excessive oxidation required to furnish the phosphoric and sulphuric acids is from the increased activity of oxidation in the lungs. It is a powerful diuretic, and has the same effect on the other glands, not excepting the salivary. The drying of the mucous membrane in the upper part of the respiratory passages, the browning of the tongue, &c., are difficult to explain. The conjunctivæ are sometimes slightly dry, often injected.

Therapeutical Uses.—A study of the physiological action of belladonna has led me to regard the plant in a new light as a curative agent. First and foremost, it is a direct and powerful stimulant to the sympathetic nervous system, or, in other words, to the heart. Secondly, it is a potent diuretic. Thirdly, by virtue of its stimulant action on the circulation, it is a means for increasing the oxidising processes within the body. Its influence as an anodyne is so fully acknowledged, that I shall omit consideration of this action upon the present occasion.

First, as a *cardiac stimulant.* It is remarkable that this, the primary and essential operation of belladonna, should have been so long neglected. This plant should stand at the head of all our stimulants; for there is no medicine in the whole materia medica which at all approaches belladonna in its simple, direct, immediate, and powerful influence in exalting the force and rapidity of the heart's action. In all conditions and diseases, therefore, in which there is a depression of the sympathetic nervous influence, such as syncope from asthenia, or shock; in the collapse of cholera; in failure of the heart's action from chloroform or other cardiac paralysers—the subcutaneous use of sulphate of atropia, in doses varying from the hundredth to the fortieth of a grain, is the appropriate and most hopeful means of resuscitation.

With a view more of ascertaining the influence of belladonna in progressive failure of the heart's action in inanition, than of hoping for a permanent good result, I injected the two-hundred

and fortieth of a grain of sulphate of atropia into the arm of an infant ten weeks old, at a time when, excepting a few beats now and then, the pulse was imperceptible at the wrist, and the cardiac systoles only 80. Within four minutes, the pulse rose to 100, and each beat was quite perceptible at the wrist. In eight minutes, it had increased to 110, and was quite regular and distinct. The stimulant continued for the next three hours; and at the end of this time the pulse was 100, of good volume, and of sufficient force to bear compression without obliteration. The respiration remained unaltered, and the pupils dilated from one-twelfth to one-seventh of an inch. The stimulant effect upon the pulse continued to within half an hour of the death of the child, five hours and a half after the injection of the atropia.

As a *diuretic*, belladonna may be used in cases of *suppression of urine*, whether accompanied by uræmic symptoms or not. As both the sluggish circulation and the torpid kidney are simultaneously aroused by the medicine, there is ground for expecting a restoration of the renal secretion.

In *acute nephritis*, we may hope for beneficial results from the use of belladonna, which, coming in contact with the irritated and congested organ, will doubtless calm the nervous irritation, and at the same time contract the dilated blood-vessels. I am at the present time busily employed in determining the effects of its operation in congested and inflammatory conditions of the kidney; and, so far as my experience goes, I am led to expect beneficial results in both states.

In *chronic albuminuria*, belladonna, I believe, will prove very serviceable, provided that the kidney has not passed into the degenerative stage bordering on fatty degeneration. In one case, Charles E., aged 35, who had been under my care for three months for an acute attack of nephritis, commencing with excessive œdema of the legs, and exudation of albumen, I administered a single dose of atropia with the following result. On the 10th of January, he had so far improved under the influence of astringent chalybeates and hydragogue purgatives, that there remained but slight pitting of the integuments over the tibia; and the urine, when boiled and heated with nitric acid gave only a small precipitate of albumen—enough, however, to render the fluid completely opaque from the presence of small flocculi of albumen. At 8 30 p.m. on the day above mentioned, I injected the forty-eighth of a grain of sulphate of atropia beneath the skin; and he passed at that time urine A. The atropia produced full effects; and at 10.30 p.m., when these had passed off, he voided urine B with some difficulty and in small driblets. Urine A had a specific gravity of 1022.4, and contained

exactly a grain of albumen in 1000 grain measures. Urine B was of specific gravity 1024.4, and contained only half the quantity of albumen present in urine A. Four days afterwards, the patient presented himself at the hospital, and reported himself quite well. The œdema of the legs was entirely gone. He passed urine in the prescribing-room; and repeated examination by my clerks and myself showed that the albumen had quite disappeared. The patient has not attended since, from which I infer that he continues well. He had presented himself regularly at the hospital the previous three months, and the urine was regularly examined. The albumen was observed to be slowly diminishing in quantity, but it never had been absent from the secretion. It appeared, in this case, that the kidneys had received a sudden impulse to healthy action.

In another case, that of John B., aged 25, who had been under my care for two years continuously for acute, passing into chronic albuminuria, the albumen, which had long been stationary, began to decrease rapidly in amount under the influence of the same treatment.

The effect of a powerful dose of atropia upon the kidneys in chronic albuminuria is well seen in the analyses before given. It will be observed that there was a decided diminution of the albumen during the operation of the medicine. The result by the operation of belladonna in these cases must be accepted as the best proof of the condition of the blood-vessels generally during that operation. It is quite clear that there is no impediment from contraction of the arteries on the one hand, or from dilatation of the capillaries on the other, to the flow of blood through the kidney. On the contrary, it appears that the vessels of the gland are aroused by the action of the drug into a healthy state of excitement; a condition highly favourable for the nutrition of the organ, and the removal of chronic disease. As a means of promoting oxidation of the blood, belladonna will doubtless prove of essential service in the uric and lactic acid diatheses. I have employed it in rheumatic fever with marked success. I inject the fiftieth or fortieth of a grain of the atropia salt into the integument over the affected joint, as soon as the first indication of inflammatory action arises in the part. The anodyne action is so direct, speedy, and enduring, that the use of opium, which, excepting for its anodyne and hypnotic actions, is decidedly objectionable in this disease, is altogether unnecessary. The subcutaneous use of atropia in other acute diseases is a wide field for inquiry, and promises, as far as my observations extend, to be a most interesting and encouraging one.—*Medical Times and Gazette, March* 21, 1868, *p.* 325, *and British Medical Journal, April* 4, 1868, *p.* 320.

111.—ON THE PHYSIOLOGICAL ACTION AND THERAPEUTICAL USE OF HENBANE, ALONE AND IN COMBINATION WITH OPIUM, AND ON THE COMBINED OPERATION OF OPIUM AND BELLADONNA.

By Dr. HARLEY, Assistant Physician to King's College Hospital.

[The following article is an abstract of the third of the Gulstonian lectures of the present year.]

The lecturer commenced by describing the effects of increasing doses of sulphate of hyoscyamia when used subcutaneously. The following may be taken as a summary :—When given to an adult, and in doses insufficient to produce dryness of the mouth, the only effects are giddiness, somnolency, and dilatation of the pupils, and a progressive retardation of the pulse to that condition in which it exists after a prolonged period of complete rest of mind and body, without diminution in its force and volume.

In doses sufficient to produce complete dryness of the tongue and hard and soft palates, there will generally be an acceleration of the pulse ten or twenty beats, with a slight increase in its volume and power. This acceleration will be observed from ten to twenty minutes after the injection. It does not usually continue for longer than twenty or thirty minutes, and rarely lasts for an hour ; dryness of the mouth comes on about twenty minutes after the injection, and continues about an hour.

In most cases there will be great somnolency, attended with so much giddiness that the patient is either unable to walk without assistance or reels about as if drunk ; the face becomes slightly flushed, and the conjunctivæ injected ; the pupils dilate. After the lapse of about an hour the mouth suddenly moistens, and the pulse, which, from the time of its maximum acceleration, had been observed to fall some five or six beats every twenty or thirty minutes, now falls with unusual rapidity, until at the end of two hours from the injection, it numbers only sixty, fifty, or even forty-two beats, still, however, retaining its original volume and power ; the giddiness and sleepiness slowly pass off, and at this time the pupils attain their maximum dilatation.

The effects are precisely the same when hyoscyamus, or its active principle, is given by the mouth. Compared with belladonna, hyoscyamus agrees with it in its effects upon the mouth and pupils. Its stimulant effect upon the sympathetic nervous system is only manifest in man in large doses, and even in this case it is comparatively transient and much less powerful than belladonna. The most prominent symptoms of the operation of hyoscyamus are excessive giddiness and somnolency, effects produced by atropia in only a very secondary degree. While

atropia is chiefly distinguished by its effects upon the sympathetic nervous system, hyoscyamine is distinguished by its influence on the cerebrum.

It would appear that hyoscyamine in combination with opium produces the most powerful hypnotic action possible. Each increases the effect of the other. Quantities of morphia and hyoscyamine, which of themselves are insufficient to produce sleep, will, when combined, speedily induce that condition.

Like atropia, hyoscyamine is eliminated by the kidneys, and the lecturer stated that he had detected it in the urine twenty-two minutes after the subcutaneous injection of $\frac{1}{15}$th of a grain of sulphate of hyoscyamine.

Treating of its therapeutical use, the lecturer stated that he had found it servicable in certain cases of epilepsy and enuresis, and extremely valuable in irritable conditions of the brain and heart, and that it is especially useful in often determining and invariably increasing the hypnotic action of opium.

In treating of the combined operation of belladonna and opium, the lecturer having previously determined the separate effects of atropia and morphia upon the horse, the dog, and man, gave the results of their operation when simultaneously administered, or when the one remedy was allowed to precede the other by a variable time.

From numerous experiments upon the horse, which were made by Mr. Frederick Mavor, of Park-street, and himself, he concluded that the reverse of any antagonism exists with respect to this animal; and he plainly proved by the experiments adduced, that the medicines not only intensify, but very much prolong, each other's effects.

The experiments upon the dog also led to the same positive conclusion. Some of them were peculiarly instructive. In one case, a quantity of atropine, which had been proved to be incapable of producing sound or continuous sleep, was given to the animal two hours after the administration of a subcutaneous dose of opium, and at a time when the dozy condition induced by the latter had passed off, the pulse being 78, respirations 18, pupils one-sixth contracting to one-seventh. Within five minutes of the injection of the atropine the animal was in a complete state of narcotism, and remained so without the slightest motion for the next four hours, and could not be aroused by pinching or pricking the skin, or by poking the finger down upon the glottis. The atropine effects, meantime, were extremely developed, and were much prolonged, and the dog continued to sleep soundly for three hours more.

In man, precisely the same results were observed in all the cases treated with opium and belladonna, either simultaneously administered, or when one was given some time previous to the other.

The lecturer could come to no other conclusion than that, as far as a hypnotic influence was concerned, belladonna decidedly increased the effects of the opium, and, on the other hand, opium invariably intensified not one or two, but *all* the effects of belladonna.

One important fact, however, resulted fram the numerous experiments which he had made upon the dog and upon man. In a large proportion of patients he found that the subcutaneous use of morphia was followed by faintness, nausea, increasing to vomiting and violent retching, with weak and often intermittent action of the heart, these distressing symptoms lasting for many hours. When, however, a small quantity of atropine (the $\frac{1}{100}$th of a grain) was administered with the morphia, these alarming effects never followed.

He explained this fact by attributing to the atropia such a powerful stimulation of the sympathetic nervous system as was able to overcome that derangement of the vagus nerve which opium so frequently produces. In other patients, in whom opium alone fails to induce sleep, the combination of opium and belladonna, whether given by skin or by stomach, procured the desired result.—*Medical Times and Gazette*, *April* 4, 1868, *p.* 376.

112.—ON THE USE OF "RICHARDSON'S SPRAY PRODUCER" IN RESTORING THE BUBBLE OF AIR TO ITS PROPER PLACE IN THE MEDICAL THERMOMETER, WHEN ACCIDENTALLY DISPLACED.

By Dr. J. W. Merriam, Valparaiso, Chili.

By careless manipulation, the self-registering principle of the Medical Thermometer may be destroyed if the bubble of air, which forms the distinctive feature of one class of these instruments, should become lost in the bulb.

This accident happened to me a few months ago, and the difficulty which I found in re-adjusting the instrument, and my final success, led me to think that a statement of the *modus operandi* might be of service to others.

While lost in the bulb, the bubble of air generally appears like a minute, bright point, and may, at first, be easily mistaken for a defect in the glass; if, however, the thermometer be reversed, and shaken a few times, the bubble rises to the top of the mass of mercury in the bulb and expands so much as to be one or two lines in diameter. It is well to satisfy one's self in this way of the exact position of the recreant bubble before proceeding further.

My first expedient was to try and restore the bubble by sudden, short jerks of the instrument downwards. This was

always sufficient to displace the bubble again, more or less, and even in some instances to cause it to re-enter the tube, but at so low a point in the column as to appear still only a mere speck on account of the weight of the column above it. After two hours' trial, I recognised the following to be the cause of the difficulty. At *ordinary* temperatures, the mercury is so much expanded that it rises too high in the tube to insure the success of the effort, and it occurred to me that, by lowering the temperature of the mass very considerably, the main obstacle would be removed. I accordingly threw a jet of *ether-spray* upon the bulb, and the mercury fell so low as to enable me, by means of a few short jerks to place the bubble where I wanted it, about five or six lines from the top of the column.

Great care must be taken not to allow the bubble to pass *above* the column of mercury, for it is doubtful whether, in that case, it could be again dislodged and placed lower in the column, where it would be serviceable.—*Valparaiso, March* 4, 1868.

113.—ON MEDICINAL INHALATIONS, WITH DESCRIPTION OF AN IMPROVED APPARATUS FOR THE PRODUCTION OF MEDICATED VAPOURS.

By Dr. James Adams, Glasgow.

[The following paper was read before the Glasgow Medico-Chirurgical Society. The combination of Bergson's and Siegle's several apparatus is the one now in general use for the purpose of medicinal inhalation.]

It consists in a small boiler heated with a spirit lamp, the capillary tubes being attached to the boiler. The steam produced in the boiler issues from one of the tubes with considerable force, and in a steady current, producing the same effect as a blast of compressed air, so that the medicated fluid is projected in a gentle warm mist or vapour, very much finer than could possibly be previously obtained by any other arrangement. Contrasting the vapour produced by this apparatus with that of the air-pump or bellows arrangement, I should liken the first to a fine Scotch mist, and the second to a plump of rain.

Experiment has shown what indeed was already very obvious, *i.e.*, that this very minute division, or pulverization, of the medicated fluids, is a matter of prime importance, as the finer the vapour the less the irritation to which it gives rise in passing the larynx, and the more effectually it penetrates to the minutest and furthest removed air-tubes. The patient requires no assistant in working the instrument; he inhales without fatigue or flurry, and at leisure; and the inhalations can be repeated and prolonged indefinitely and at pleasure.

I have during the last fifteen or eighteen months had very extensive trials of this apparatus; and I think that, with some modifications it has undergone at my hands, and to which I will immediately refer, it leaves little to be desired. I was led to a consideration of these modifications on account of the high cost of the instruments, ranging from 15s. to 50s., and the occasional delay of many days, and even weeks, before they could be obtained. These were serious obstacles to a fair trial, or to the general use of the instrument, and I was unable to stimulate either instrument-maker or druggist to find a remedy. I at length got a smart tinsmith to work several patterns under my directions, and to produce an instrument equally effective with the most costly, and a respectable druggist to give him an order for a supply. These within the last few months, under the stimulus of the sulphur mania, have been sold in large numbers by several druggists at an average cost of 4s. One druggist tells me he alone has sold upwards of 200.

I felt interested in my attempts to improve the instrument, and you would be amused if I showed you all the patterns from first to last, and surprised if I were to go into a description of little details which required consideration or remedy before the instrument I now place on the table was produced. Suffice it that I feel warranted in saying that it is the most efficient, ready, and cheap instrument that can at the present moment be obtained. Let me state shortly some of the qualities wherein I think that it contrasts with, and is superior to, others.

It is compact and ready for use, and is so put together that there is no necessity for frequent and nice adjustments,—consequently the risk of troublesome disarrangements, or of accidental injury of the instrument, is exceedingly small.

The form of the boiler is distinctive and peculiar, and ensures several advantages. Thus, the position of the water inlet enables the boiler to be filled to the proper height and no more. Above the water line and inlet there is a reservoir for steam sufficiently large to maintain a continuous current of spray, and to project it to any distance that may reasonably be desired. This arrangement of the water inlet, steam chamber, and steam outlet, prevents a very annoying and even dangerous accident of frequent occurrence in instruments with the ordinary form of boiler, namely, the forcible projection of spirts of scalding water in the face of the patient, caused by the boiling liquid coming over with the steam. The heat from the spirit lamp is carried up through the centre of the boiler, thus reaching a larger heating surface of the boiler, generating steam more rapidly, keeping up a full supply of the steam, and at the same time *super-heating* and *drying* the steam so generated. The steam escapes by a short horizontal nozzle at the top of the

boiler, and necessarily is subjected at the instant of its escape to the action of the flue of the lamp, thus ensuring such a dry condition of the steam that it quickly becomes dissolved or dissipated in the air, so lessening the risk of annoyance to the patient, and at the same time avoiding in a great measure the dilution of the medicated fluid with watery steam. By carrying the flue of the lamp through the centre of the boiler, the body of the instrument is not so hot but that a handle attached to the lower and cooler portion of the case can be grasped with comfort and safety even when in use. An alarming-looking and costly safety-valve, very liable to become stiff and unworkable, has been dispensed with, as I have found that a simple cork or india-rubber plug is equally efficient and more convenient. The box containing the phial of medicated fluid is soon warmed by the mode of its attachment to the case of the instrument, and there are other details of minor importance which, when combined, make the apparatus more convenient in use.

Before parting with my reference to this instrument I may mention that I have experienced annoyance from finding my patterns and directions departed from occasionally by workmen who did not understand what I was aiming at. An old friend of mine, who had opportunities of knowing this fact, suggested a patent, which I of course rejected, my ideas on medical patents being at one with my friend Dr. Gairdner. I did not object, however, that he should do so ; and, associating with himself a respectable druggist, he has taken out a patent. I hope thus to ensure accuracy and good quality of workmanship with cheapness of manufacture. *This will certainly gratify me; but beyond this, I beg emphatically to say that I have no right of property in the instrument, and no interest whatever in its sale.*

Having said so much regarding instruments, I have little time to refer to the medicaments, which may conveniently, and with probable advantage, be employed in the form of vapour. Those with the use of which I am most familiar are solutions of morphia, digitalis, stramonium squill, tannin, alum, nitrate of silver, sulphate of zinc, chloroform, acetic acid, and sulphurous acid. I extend and combine this list of agents as seems to me desirable. Each drug has of course its special properties, and it would open too large a discussion to enter, however shortly, upon their consideration. I might say much to show why I have reason to be satisfied with the results I obtain from time to time ; but I think it sufficient to indicate the fact that I am so satisfied, and that I believe that the inhalation of medicated vapours is likely to be a more familiar and a more important therapeutic agent in the hands of the physician in time to come.

Those who are inclined to pursue this subject will find ample details of the history and full directions for, and illustrations

of, treatment by medicinal inhalations in the work of Dr. Biegel, of London—an admirable work in my opinion, and showing in its author the possession of sound common sense and professional ability.

At the last moment it has occurred to me that it might give point to the object of my communication, and open an easy inlet to the observations of the members, if, before concluding, I should make special reference to one agent, with the name and alleged miraculous virtues of which the public of Glasgow has been made very familiar during the last few months. I need scarce say that I allude to sulphurous acid gas, or, as it has been termed in the popular epistles through which it has gained notoriety, "the Great Sulphur Cure."

The statements in Dr. Dewar's original pamphlet regarding the value of sulphur as a great remedial agent, seemed to me at the time sufficiently moderate and circumstantial to warrant a candid examination; and during the last twelve months, and of course long before the recent excitement, I have given to it a fair trial in a spirit of patient observation. I may at once say that I have satisfied myself that it has its uses—that it is not an innocuous agent—one to be pooh-poohed and laid aside without trial, or to be contemptuously stifled in a cloud of "chaff" scattered over the columns of a newspaper. But, to qualify this allusion, I admit that it is very difficult to be amazed and yet temperate when reading the loose statements, crude deductions, and far from satisfactory cases recently published by Dr. Dewar; or, to refrain from disparaging the rhapsodical style of his enthusiastic follower, Dr. Pairman. I am well satisfied that they are both sincere men; and, believing as I do, that they have made statements in perfect good faith, and alleged as matters of fact, occurrences that are open to daily and familiar experiment and observation, I think it preferable to sift these statements before troubling myself about misty theories, or rather hypothetical conjectures—or in denouncing pretensions however apparently absurd. If these gentlemen are competent observers, and of sound judgment, then the occurrences they have recorded must have been repeated under like circumstances; and this recurrence of powerful and novel effects has no doubt been observed, and will be corroborated by the testimony of medical men whose position as competent observers is established. We have such observers among us this evening, and I hope they will express such a judgment as their experience warrants them in giving. As regards myself, I repeat that I have observed with patience and without prejudice, and as a result of my observations I felt warranted, at the commencement of the late sulphur mania—for it deserves no other name—in expressing my opinion that the agent was useful; and that

Dr. Dewar deserved great credit for his earnest and persevering efforts in directing the attention of the profession to several novel and useful applications of the remedy; but that it was most preposterously over-lauded—that its excessive popularity would have a short day—and that some injury and many grievous disappointments would remain *en souvenir*.

My experience of the remedy, in some of its principal applications to medicine in the way of inhalation, and without any reference to surgical ailments, may be shortly stated.

Individuals whose general health was good, and who have resorted to the sulphur inhalations on account of such slight forms of catarrah us usually receive little or no medical treatment, except it may be a sweating powder, or a warm bath, have been the most liberal in their acknowledgments of benefit. It did really seem to me that some cases were relieved or shortened in their progress by the treatment.

In acute bronchitis I have seen a copious secretion of serous or watery fluid cast off from the air passages at an unusually early stage of the disease, and this effect was evidently induced by the inhalations, and was followed by marked relief.

In chronic coughs it has frequently acted powerfully in exciting forcibly expiration, and in inducing a more copious expectoration.

From these results I am of opinion that in catarrh, acute bronchitis, and chronic coughs, the remedy stimulates the minute exhaling vessels, the bronchial surface seems to be both sweated and purged, and the tough, viscid phlegm which collects in the bronchi is dislodged more freely and effectively than occurs under the use of ordinary expectorants, administered in the usual way. But I have found no notable difference nor any advantage in use in the sulphurous acid over common vinegar or solution of chlorine administered by inhalation in like cases.

In a few opportunities I have tried it in asthma, but have not found that the inhalations were tolerated or continued, partly on account of marked distress which they occasioned, and partly because no evident relief was obtained during the paroxysms.

In phthisis its effect seems to be altogether that of a topical expectorant, but I have not seen any decided lessening of the amount of expectoration.

Cases of inflammatory sore throat have not been benefitted, but in the hoarseness following the acute stage of a cold I have seen improvement and relief.

I have not seen, in any of its applications, the alleged "sedative" or "calmative" action of the remedy.

Troublesome and even alarming consequences are of occasional

occurrence after prolonged inhalations of the dry sulphur fumes, or of the aqueous solution of sulphurous acid gas in its full strength,—such as great tumefaction of the tonsils and pendulous palate, husky voice,—difficulty in swallowing,—pain in larynx spitting of blood, &c.; and I have seen all these effects occur under circumstances where no blame could be justly imputed on the score of carelessness.

It is not, therefore, a remedy to be administered indiscriminately or without precaution, and I deprecate the practice of fumigating a patient's room by throwing a quantity (no limit as to quantity) over a heated shovel or live coal, and filling an apartment (which may be large or small, well ventilated or otherwise) with the dry fumes of an indefinite quantity of sulphurous acid gas,—an agent of known powerful chemical properties, and alleged to possess very potent influence upon the living organism. As well tell a patient to take "a large dash of laudanum" in his gruel when going to bed, or "a good pinch of calomel" occasionally.

My remarks on the sulphur cure have gone to a greater length than I anticipated, and I will now only add that I have not seen any effects from its use that have led me to think that it has any *specific action, when inhaled in any disease*, or that it has any other effect than that of a local stimulant, tonic, and astringent. As such I believe it is likely to prove a useful adjuvant in the treatment of various maladies.—*Glasgow Medical Journal, March* 1868, *p.* 401.

114.—ON THE ANÆSTHETIC ACTION OF BICHLORIDE OF METHYLENE,

By Dr. Benjamin W. Richardson, F.R.S.

I wish you to observe the action of the anæsthetic again, inasmuch as it is typical of all the group of the chlorides of the monocarbons. To see the symptoms well, we will narcotise slowly. A pigeon is again the subject: it is the hundred-and-thirteenth time this bird has been narcotised by the agent, but it has never suffered in the least. By narcotising slowly, we develope all the stages or degrees of sleep—a first degree of slight excitement; a second degree (and in this case very short, not more than a second or two in duration) of rigidity; a third degree of relaxation and complete insensibility; and, pushing the inhalation a shade further, that extreme degree, fourth degree, of prostration, which is immediately preliminary to death. We now remove the animal; it will remain unconscious, probably, for half an hour or more, and will then suddenly recover.

I say this substance is a fair type of all its allies, and this is true; but it has certain different points of action, which mark it off. It narcotises more quickly in an open chamber than the chloride of methyl, because its vapour is so much heavier—42·5 to 25·25—and is therefore less readily diffused in the air and lost, and it narcotises more quickly than chloroform, or tetrachloride of carbon, because its vapour, quite heavy enough for inhalation, and lighter than the vapour of chloroform in the proportion of 42·5 to 59·75, and lighter than the vapour of tetrachloride of carbon in the proportion of 42·5 to 77, is more easily diffusible from the air vesicles of the lung into the blood than its heavier comrades, for reasons we shall see in the sequel.

Before I leave the bichloride of methylene, let me say a word in reference to its action on the human subject. I have now administered the bichloride for the performance of nine capital operations. In one case the insensibility was sustained for an hour and seven minutes; in no case was the insensibility less than thirty-five minutes. In all there was the same order of phenomena as that we have just seen—a brief first and second stage, a rapidly developed third stage, and a very prolonged and deep anæsthesia. The same results have been obtained by my friend, Mr. Peter Marshall, one of the earliest students and practitioners in anæsthesia, and by Dr. Junker.

In one of my cases only was vomiting a symptom during the anæsthetic condition, and in that case the vomiting was not, I think, strictly attributable to the narcotic; it was also very slight, a single effort with eructation causing the loss of about a table-spoonful of fluid from the stomach.

The prolonged anæsthesia after free inhalation of the bichloride of methylene is a striking feature in its action, and has, I understand, been a cause of some anxiety. There is no occasion for anxiety from, nor for interference with, this sleep; it passes off quite naturally if time be allowed, and what is more, it passes off leaving no headache and no feeling of depression behind. In most cases of operation, the prolonged anæsthesia is indeed of advantage, the sleep being so quiet, so undisturbed, so gentle, and so easily sustained by the briefest renewal of the administration of the vapour. I had an opportunity of obtaining a comparison from one of my patients respecting the difference of effect between chloroform inhalation and inhalation of bichloride of methylene. This patient, some years ago, was narcotised with chloroform by Dr. Snow; she was narcotised with bichloride of methylene by myself, to enable Mr. Spencer Wells to perform the operation of ovariotomy. She was a very acute observer, and most precise in her definitions. A few days after the operation Mr. Wells was so good as to take me to her, when she explained her

differences of sensation under chloroform and under the bichloride of methylene. Her sensations under chloroform as she began to inhale it were those of oppression of the breathing, loud noises in the head, ringing in the ears, and a feeling of absolute necessity to get up and resist the further administration; on recovery from the chloroform, she felt an indescribable sense of exhaustion, an intense nausea, and a headache which lasted for many hours. By the inhalation of the bichloride of methylene she escaped, she said, every one of these annoyances. She dropped into sleep as into natural sleep; she awoke as out of natural sleep, with not one painful symptom; in all, it was as though she had closed her eyes and opened them again, the intervening space of time during which the operation had been performed, and which, with after-sleep, had reached fifty-five minutes, having been altogether inappreciable to her mind.

With the facts I have carefully collected before me, I have no hesitation now in stating that bichloride of methylene as a general anæsthetic is at least as safe as chloroform, and that it possesses certain advantages of its own which give it a better position amongst anæsthetics than chloroform. The only obstacle to its success is the difficulty of its manufacture and its consequent greater cost than the cost of chloroform; but I should trust that in time this obstacle will be overcome. Administered internally by the stomach in doses of from ten to twenty minims, bichloride of methylene is a good anodyne. To make our studies complete, we have let an animal—a rabbit—sleep to death in bichloride of methylene. The action of the heart and of the muscles of respiration ceased at the same moment. I will pass the dead animal round, and you will see that the lungs are of natural colour, and that the heart contains blood on both its sides. In the trough with the animal is some blood saturated with the bichloride. The colour of the blood is bright red, but the fluid has undergone a feeble coagulation. The coagulum is not of fibrin, for the blood was defibrinated at the time it was abstracted, it is a clot of albumen and red corpuscles. This is a peculiarity of action of all these chlorine compounds; the cause coagulation of defibrinated blood. In this direction the bichloride is less effective than chloroform, and chloroform is less effective than tetrachloride of carbon, but the tendency is distinctly indicated in the specimen under observation.—*Medical Times and Gazette, Dec.* 28,1867, *p.* 693.

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